

JAMES W. MARCHAND

THE SOUNDS AND PHONEMES  
OF  
WULFILA'S GOTHIC

MOUTON



## THE SOUNDS AND PHONEMES OF WULFILA'S GOTHIC

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# THE SOUNDS AND PHONEMES OF WULFILA'S GOTHIC

*by*

JAMES W. MARCHAND

UNIVERSITY OF ILLINOIS

*Sunt bona, sunt quaedam mediocria, sunt mala plura,  
quae legis hic: aliter non fit, Avite, liber.*

1973

MOUTON

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## INTRODUCTION

No one would, I think, deny the need for a new study of Gothic pronunciation. The last treatment which purported to be in any way complete was that of Dietrich in 1862.<sup>1</sup> Since that time linguistic science has undergone two major revolutions. Methodological refinements have been made in every phase of our study. Besides this, new material has been collected and many vexing points have been cleared up. We are in a better position to discuss the loans, the origin of the alphabet, Proto-Germanic and the internal evidence. Almost all authors of previous studies, those concerning the sound values of single signs as well as those treating the pronunciation of several signs, have operated with an implicit methodology. The use of implicit methodology and the appeal to “common sense” always make for ill-defined and ill-conceived ideas. As is often true in such cases, much has been assumed for Gothic pronunciation on flimsy or non-existent evidence. There is a need for a treatment which will operate with an explicit methodology and which will employ the requisite scepticism. Such a new study, carried out with a better regard for methodological soundness may well yield results which are at variance with the usual concept of Gothic pronunciation. If it does not do this, if it merely confirms the studies previously made, it will serve the useful purpose of placing our ideas on a sounder footing methodologically speaking.

But a treatment of Gothic pronunciation for the sake of Gothic pronunciation can only be a sterile exercise in logic. One may well ask, and it has not been often enough asked in the past: what is the value of such a study? Such a study has a two-fold value: it raises points of methodology which may have applications elsewhere, and it affords us a solid basis for our comparative study. No one can deny the need for the first. I don't think that any serious scholar can deny that the first prerequisite for historical or comparative phonology is a synchronic and syntopic description of the sound features of the languages or stages of the language to be compared. Many of our ideas concerning the sound features of Proto-Germanic are based on our idea of how Gothic was pronounced. If the latter changes, the former must of necessity change (cf. sect. 5.3). Very few linguists operating in historical and comparative linguistics in America today would be willing to accept the premises upon which the

<sup>1</sup> F. Dietrich, *Über die Aussprache des Gothischen während der Zeit seines Bestehens* (Marburg, 1862).

common idea of Gothic pronunciation is based, yet they operate with the sound values assigned to Gothic signs in the handbooks. To do this is to base one's conclusions on premises one cannot accept. Since diachronic study depends in the last analysis on synchronic study, a reinterpretation of our synchronic data may lead to a reinterpretation of some points of our diachronic analysis (see sect. 5.31).

Recent refinements in methodology, the phonemic principle, historical phonemics, internal reconstruction, are well-suited to aid us in this reinterpretation. And, as is always true, the application of new methodology will bring with it new insights into old problems. It may be that the application of the methodological refinements mentioned above will afford us a clearer picture of the proto-language and pre-historic changes than before.

Traditionally, the following four criteria have been used to determine the pronunciation of Gothic: (1) the origin of the alphabet; (2) loan words and transcriptions of proper names; (3) internal evidence; (4) comparative evidence.<sup>2</sup> Sievers and Streitberg used the technique known as *Schallanalyse*, but this is merely a technique for operating with internal evidence.<sup>3</sup> Trubetzkoy used his theory of vowel systems to determine certain points of Gothic phonology.<sup>4</sup> This also is merely a technique for operating with internal evidence. Criterion (4) is the one which usually receives the most emphasis, and the three others are often considered as less important than it. This is methodologically unsound, as is pointed out below (Chapter 5). In the usual treatment all four are lumped together and discussed, as it were, in the same breath. As it is almost impossible to avoid letting one criterion influence the judgement of another under such circumstances, it is better to discuss each criterion as an autonomous unit.

The thesis follows the following order: Chapter 1 discusses criterion (1), Chapter 2, criterion (2), Chapter 3 and Chapter 4, criterion (3), and Chapter 5, criterion (4). The order is not fortuitous, not do I believe that it could be changed. It is proper to start out with the only direct evidence we have, the alphabet. We need to discuss the loans in order to know whether Wulfila used his signs in values close to the original values, or if he disregarded these in his choice. It is perhaps possible to discuss internal phenomena without considering the sound values of the signs involved, but we must have some idea of the sound values to make these phenomena meaningful to us. It is, of course, a truism that we need a synchronic and syntopic analysis before attempting a comparison. We must have something to compare.

This book, then, is an attempt to study the sound values of Wulfila's Gothic through the use of new techniques and a more rigorous application of old techniques.

<sup>2</sup> Cf. Streitberg, p. 53 f.; Braune-Helm, p. 4; Jelinek, p. 30.

<sup>3</sup> For a discussion of the methodology of *Schallanalyse*, see G. Ipsen and F. Karg, *Schallanalytische Versuche* (Heidelberg, 1928). Sievers' investigations were approved by Streitberg, who used them in the 5th and 6th editions (one edition) of his grammar. Sievers has prepared the Sermon on the Mount using his technique (*Deutsche Sagversichtungen des IX-XI Jahrhunderts* [Heidelberg, 1924], pp. 164-173).

<sup>4</sup> "Zur allgemeinen Theorie der phonologischen Vokalsysteme", *TCLP*, I, p. 57.

## THE ORIGIN OF THE GOTHIC ALPHABET

## 1.0 METHODOLOGICAL REMARKS

All our Gothic material is written in a special alphabet.<sup>1</sup> We are told by ancient authors that this alphabet was devised by Wulfila, the Gothic bishop to whom the feat of translating the Bible is also ascribed.<sup>2</sup> Since we are in no position to doubt these statements, they have found general acceptance and are here assumed to be true. We have to do then with an alphabet devised by a single man, a scholar, who was proficient in at least three languages, for use in his Bible translation.<sup>3</sup> This translation was presumably made for the purpose of disseminating Christian doctrine among the Goths.<sup>4</sup> It was the custom in those days that the Bible was read to the congregation by a special reader, and this custom is also attested among the Goths.<sup>5</sup> So Wulfila doubtless intended his translation and his alphabet to be used by Gothic clerics who perhaps knew no other language.<sup>6</sup> It is in this light that we must look upon the Gothic alphabet.

<sup>1</sup> Except perhaps for the Gothic notes found in the *Codex Veronensis*; cf. C. von Kraus, "Gotica Veronensia", *ZfdA*, LXVI (1929), p. 213. Gothein, "Zu den Gotica Veronensia", *ZfdA*, LXVII (1930), p. 207, is of the opinion that the Gothic alphabet is used here also.

<sup>2</sup> Philostorgios (Streitberg, *Bibel*, xx): "καὶ τὰ τε ἄλλα αὐτῶν ἐπεμελεῖτο καὶ γραμμάτων αὐτοῖς οἰκείον εὐρετὴς καταστάς, μετέφρασεν εἰς τὴν αὐτῶν φωνὴν τὰς γραφὰς ἀπάσας." Sokrates (Streitberg, *Bibel*, xxi): "τότε δὲ καὶ Οὐλφίλας ὁ τῶν Γότθων ἐπίσκοπος γράμματα ἐφεῦρε Γοτθικά καὶ τὰς θείας γραφὰς εἰς τὴν Γότθων μεταβαλὼν". Sozomenos (xxiii): "πρῶτος δὲ γραμμάτων εὐρετὴς αὐτοῖς ἐγένετο καὶ εἰς τὴν οἰκείαν φωνὴν μετέφρασε τὰς ἱερὰς βίβλους." It has been remarked that it is strange that Auxentius, Wulfila's foster son, did not mention this feat of Wulfila. Since Sozomenos has copied Sokrates (Streitberg, *Bibel*, xx), it is incorrect to speak of three sources here. It may be that the story of Wulfila's "invention" of the alphabet goes back to one source. An investigation of this point cannot be made here, but the point should be raised.

<sup>3</sup> Cf. above, footnote 2, on Wulfila's use of the alphabet in his translation. We are told by Auxentius that Wulfila preached forty years "... in episcopatu gloriose florens apostolica gratia grecam et latinam et goticam linguam (Streitberg, *Bibel*, xvi)."

<sup>4</sup> We are specifically informed by Sokrates that this was Wulfila's intention (Streitberg, *Bibel*, xxi): "... τοὺς βαρβάρους μαρθάνειν τὰ Θεῖα λόγια παρεσκεύασεν."

<sup>5</sup> On the border of Ambros. B we find 44 places set off by the word *laiktjo*, i.e. "reading text" (Streitberg, *Bibel*, xxvii). The fact that some mss. are written in *stichedon* style bespeaks the fact that they were intended to be read aloud. Auxentius reveals that Wulfila himself was a "lector" before being consecrated bishop (*ibid.*, xvi). Cf. further Fr. Kauffmann, "Beiträge zur Quellenkritik der got. Bibelübersetzung", *ZfdPh*, XLIII (1911), p. 404.

<sup>6</sup> We must assume that some of the Gothic clerics knew Greek and Latin, especially those who were, like Wulfila, of mixed parentage. We know that Sunnia and Frethela and the writers of the preface to the Codex Brixianus knew Latin.

From the inception of Gothic studies, the alphabet has been seen as one of the best keys to the pronunciation of Gothic. It was felt that, if we could determine the origin of each sign, we could reasonably assume that Wulfila would have used it in the value it had in the original language. It was in this spirit that the first studies of the Gothic alphabet were conceived.<sup>7</sup> More recently, however, the alphabet has come to be studied as an end in itself. Without scholars having realized it, the problem has shifted, so that the main point has become the determination of the "rules" which governed Wulfila's choice of signs.<sup>8</sup> This unreasonable attitude does not take into account two things: we cannot be sure that the signs found in our mss. are Wulfila's signs, as we shall see below; even if we could be sure of this, we should be unable to fathom the workings of Wulfila's mind. If psychologists cannot predict what a present-day individual will do, they would find it infinitely more difficult at a remove of fifteen centuries. We must forget, as it were, all we know about Wulfila in studying his alphabet and its origin. It is not in a study of Wulfila that we must seek the origin of his signs. The safest and the most logical way is to take the forms of his alphabet which we find attested, try to determine which is the archetype or reconstruct an archetype, and compare this form with the alphabets he could have known. This rule we must at all times observe: we must be able to derive all variant forms from the form which is assumed to be the original or closest to the original.

### 1.01 *The Alphabet*

The Gothic alphabet, as it is found in the handbooks and in the hand of Scribe II of CA (the scribe of Mark and Luke), has the following form and numerical value:<sup>9</sup>

ᚠ — 1; ᚢ — 2; ᚦ — 3; ᚪ — 4; ᚥ — 5; ᚷ — 6; ᚹ — 7; ᚱ — 8; ᚰ — 9; ᚱ — 10; ᚳ — 20; ᚴ — 30; ᚾ — 40; ᚿ — 50; ᚷ — 60; ᚱ — 70; ᚱ — 80; ᚱ — 90; ᚳ — 100; ᚳ — 200; ᚳ — 300; ᚱ — 400; ᚳ — 500; ᚳ — 600; ᚱ — 700; ᚱ — 800. In most

<sup>7</sup> For a complete bibliography of earlier works, see Mossé, pp. 280-282. By earlier I mean here works before 1860.

<sup>8</sup> Von Friesen, "Gotische Schrift", *Hoops' Reallexikon*, II (1913-1915), # 7f. E. Hermann, "Ulfilas Alphabet", *NGWG* 1930, Phil.-hist. Klasse, p. 137: "Nach welchen Grundsätzen ist Ulfila verfahren?" Bouuaert "Oorsprong en vorming van het Gotisch Alphabet", *Revue belge de philologie et d'histoire*, XXXVIII (1950), *passim*. Jellinek, p. 26, for example: "... und zwar ersetzte er, solange es anging, jede Rune durch einen griech. Buchstaben; wo dies nicht möglich war, griff er zum lat. Alphabet oder behielt die Rune bei". These examples could be greatly increased.

<sup>9</sup> It is of course a failing in the handbooks to call this form "the Gothic alphabet"; it would be better to indicate which form of the Gothic alphabet is being used, as Jellinek does (p. 22). In such cases, however, we should be certain to give only forms which are actually attested, and not forms which are convenient to the printer. I found no case of such a  $\mathfrak{t}$ -sign as that cited by Jellinek (p. 22) in the photographic copies of CA available to me. If we are not going to use actual forms, a transcription would be better, since even scholars may be led astray by such a form as  $\psi$  for  $\mathfrak{p}$ . In making the forms of this hand, I followed the practice described by Fairbanks and Magoun, "On Writing and Printing Gothic", *Speculum*, XV (1940), pp. 315-322. Since the use of a Speedball C-3 pen, suggested by them, would have resulted in letters too large for my text, I have used a Speedball C-4, reducing my other dimensions proportionately.



handbooks a form  $\ddagger$  — 900 is also found. This latter practice is misleading and should be given up, since this form does not occur in our mss. of the Bible, but only in the Salzburg-Vienna ms., which is of the 9th century.<sup>10</sup> The alphabet as given above is the point of departure for the traditional treatment of the origin of the Gothic alphabet. There are several things, however, which militate against the use of this form of the Gothic alphabet for determining its origin.<sup>11</sup> This hand is written with a straight pen.<sup>12</sup> This form of writing was not practiced in the 4th century and cannot have been known to Wulfila.<sup>13</sup> There is a variant "hand" found in some mss. and in the deeds.<sup>14</sup> The difference is so striking that some authorities speak of two different hands, though others would merely consider the difference to be one in *ductus*.<sup>15</sup> The usual treatment is to dismiss this "hand" as a later development from the CA hand.<sup>16</sup> This must, however, first be determined, and several points speak against it. All authorities are united in considering the Greek alphabet to be the basis for Wulfila's alphabet.<sup>17</sup> Hand II (the CA hand) betrays Latin influence, how-

<sup>10</sup> Especially the practice of citing  $\ddagger$  in the straight pen form (cf. for example Braune-Helm, p. 2), since this is not attested. In the O.T. fragments where this form might have been found (Neh. 7, 39), we find instead *nium hunda*. In the dating of the manuscript we are following T. Sickel, "Alcuin-studien", *Sitzungsberichte ... in Wien*, LXXIX (1875), p. 471 ff. This article has not been noticed by Germanic scholars (it does not figure in Mossé's bibliography), but it is deserving of notice. He gives a detailed analysis of the hands and the alphabet. His method of dating cannot be reproached.

<sup>11</sup> This unreasonable attitude is merely a concomitant to the unreasonable worship of the CA traditional in Gothic studies. Thus, for example, in spite of the fact that Kauffmann ("Zur Textgeschichte der got. Bibel", *ZfdPh*, XLIII, [1911], pp. 118-132) has given good reason to believe the *stichedon* style of Car. to be more original than that of CA, textual critics continue to neglect Car. and to accept the style of CA as the more original.

<sup>12</sup> In this style the nib of the pen is so held or cut that it is at all times during the writing parallel with the horizontal line of the paper. The result is a hand in which thick vertical strokes and thin horizontal strokes regularly alternate with a gradual transition in between.

<sup>13</sup> Edward Johnston, *Writing, Illuminating, and Lettering*, 13th ed. (London, 1923), p. 43. Bouuaert, *op. cit.*, p. 424. As may be seen by the photographic facsimile in Helm and Glaue's article on the Giessen fragment ("Das got.-lat. Bibelfragment der Univ. zu Giessen", *ZfnW*, XI [1910], pp. 1-38), this fragment is written with a slant pen.

<sup>14</sup> This is a fact all too seldom recognized. Older scholars seem to have taken it for granted (cf. Gabelentz and Löbe II, II, 14 and Massmann, *Die Urkunden von Neapel und Arezzo* [Munich, 1837], p. 6), but more recent studies only mention it in passing, if at all.

<sup>15</sup> This point of view may be traced to Massmann (*loc. cit.*), who calls this hand a "liegende, laufende Schrägschrift" as opposed to the "feste, stehende Schrift" of the CA hand. It is continued by Gabelentz and Löbe, Jelinek ("Der Duktus ist nicht in allen Handschriften gleich ...", p. 21) and others. Bouuaert, who is usually unreliable in such matters, calls this a cursive (!) script (*op. cit.*, p. 424, footnote). I prefer, of course, to follow the more experienced viewpoint of Fairbanks and Magoun (*op. cit.*, p. 315) and call it a different hand. Following these two scholars, I call the hand of the deeds "Hand I" and the hand of CA "Hand II".

<sup>16</sup> Cf. Bouuaert, *op. cit.*, p. 424, footnote. The only scholar who takes this hand into consideration in his treatment is von Friesen, "Got. Schrift", # 6 and # 1: "... hinsichtlich dreier Buchstaben zeigen diese Dokumente Formen, die von besonderer Bedeutung für die Auffassung vom Ursprung des got. Alphabets sind". He is, however, not consistent, nor does he give all the variants. In a more recent work (von Friesen and Grape, p. 88), von Friesen admits that Hand I may be closer to the original.

<sup>17</sup> Sporadic attempts are made to derive it from other alphabets. The latest of these, by P. Akinian, which I unfortunately have not seen, tries to derive it from the Armenian alphabet. This must fall before the obvious fact that Wulfila is earlier by a century than the creation of the Armenian alphabet

ever, by its suspension of both final *-n* and final *-m*,<sup>18</sup> and by its use of the form *ſ*, whereas Hand I shows Greek influence in its suspension of final *-n* alone and the sigma-like *ε*-form. Hand I also exhibits forms which are non-Wulfilian (*ſ*, for example), so we need not argue the case as to which is the older form here.<sup>19</sup> It is not important which hand is earliest attested, as almost any comparative linguist or textual critic will affirm. Since we have two hands, each of which may exhibit forms which have changed in the two centuries between Wulfila and our mss., it seems the proper course is first to attempt a reconstruction for each form under discussion, and then to try to connect this reconstructed form with other alphabets.<sup>20</sup> As there is some evidence of a continuous use of the alphabet among the Goths, particularly in Italy,<sup>21</sup> it seems unlikely that this will have remained unchanged in the interval of two hundred years.

### 1.02 *Alphabets from which borrowing was possible*

The first problem in discussing the origin of the signs is to determine the alphabets from which Wulfila could have borrowed his signs. The fact that Wulfila knew Latin, Greek and Gothic, and that he wrote tractates in these three languages, does not tell

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(cf. von Friesen, "Got. Schrift", # 4). Besides the obvious similarity of many letters, the agreement in the use of letters for numbers bespeaks a Greek basis.

<sup>18</sup> This was first remarked by von Friesen and Grape (p. 87). Cf. also Fairbanks and Magoun, *op. cit.*, p. 316. The failure to notice this trait, which mars all the treatments before Fairbanks and Magoun, and which is not taken cognizance of in later articles (by Gutenbrunner and Bouuaert), is a concomitant to the fact that the alphabet has been treated to a great extent only by amateurs in the field of palaeography. As Prof. E. A. Lowe has recently said ("The Oldest Omission Signs in Latin Mss.", *Miscellanea Giovanni Mercati*, vol. 6 [Vatican, 1946], p. 71): "There is no clue too insignificant for the palaeographer. By paying attention to such trifling details as the omission marks used in a ms. he may be enabled to draw specific conclusions as to the home of the exemplar or archetype or gain some new light regarding his manuscript's later vicissitudes".

<sup>19</sup> Fairbanks and Magoun, p. 316, consider the hand of the deeds, with good reason, to be "doubtless the older". It seems strange that Bouuaert, writing in 1950, did not see fit to give his reasons for rejecting their viewpoint, but affirmed that the sloping uncial "quite probably stems from the Gothic uncial (!) and may have undergone the influence of the Latin script (p. 424, footnote)". Von Friesen and Grape also admit this possibility (Cf. above, p. 15, footnote 16).

<sup>20</sup> An improvement over the traditional treatment (with the exception of that by Gabelentz and Löbe) would be merely a statement of the forms which occur. Such a statement was at first prepared for this thesis, but it was felt that it was not in keeping with its aim to devote so much space to the alphabet. It is to be hoped that a complete treatise on this subject by a palaeographer will be forthcoming in the near future.

<sup>21</sup> There is evidence that Theodoric himself used the Gothic alphabet in the story of an anonymous historian (found in the *Excerpta* printed at the end of Gronovius' edition of Ammianus Marcellinus, 1693, p. 512). It is said that Theodoric used a quill pen and a stencil to form the four letters necessary to sign his name. Since another source tells us that it was THEOD which the king signed (cf. Gabelentz and Löbe, p. 15), we cannot accept Thompson's theory that Theodoric merely signed *legi* (E. M. Thompson, *Introduction to Greek and Latin Paleography* [Oxford, 1912], p. 40, without noting that it was originally Reiske who put forth this view). Since *Theod* would have been five letters, the first letter must have been *thorn*. Of course, most of our manuscripts are written in Italy, and the deeds attest to the use of the Gothic alphabet in business matters.

us what alphabets he knew.<sup>22</sup> We do not know enough about his life to be sure what alphabets he might have seen. We are able to use another criterion to narrow the field down, however; we can be sure that he must have used one of the alphabets derived from the Greek, since he seems to have used signs for vowels.<sup>23</sup> Since the Armenian and the Cyrillic alphabets were devised after Wulfila's alphabet, we must exclude them from our considerations. We are then practically restricted to the Greek, Latin (including Italic), and runic alphabets, but for a different reason than the one usually given.

### 1.03 *The value of a study of the alphabet for determining pronunciation*

We must be careful to avoid an error into which previous investigators have fallen. This consists in attempting to use the alphabet to determine pronunciation, while at the same time using the pronunciation to discuss the origin of the alphabet.<sup>24</sup> If our discussion of the origin of the alphabet is to have any value for our discussion of Wulfila's pronunciation, we must exclude discussion of the latter until we have completed discussion of the former. Any assumed pronunciation of a sign cannot be used in the discussion at hand.

Our previous discussion has centered on the determination of the origin of each sign. There is another problem which has not been clearly conceived nor stated in previous studies, though it is mentioned in some. If we succeed in determining with certainty the origin of each sign, can we be certain that Wulfila will have used each sign in exactly the same value it had in the original language? The answer must be an emphatic negative. The relationship between the sign and the sound it represents is arbitrary for each system, often for each writer; when the sign is applied to the sounds of another language, this bond is of necessity broken.<sup>25</sup> This statement must not be

<sup>22</sup> It is often affirmed that this fact implies a knowledge of the Latin, Greek and runic alphabets (von Friesen, "Got. Schrift", # 4; Hermann, p. 136). When we consider the fact that many learned men of Wulfila's time were unfamiliar with the writing systems of their own languages, we cannot affirm with certainty that Wulfila knew the writing systems of all those languages which he used. Runes were not used by the Goths for profane purposes, but were the property of the rune-masters, whom we so often find named on inscriptions. The first known example (and this is none too certain) of the use of runes for profane purposes is found in a letter of Venantius Fortunatus, who writes a friend: "... du magst auch barbarische Runen auf Täfelchen aus Eschenholz malen ...", as an admonition for not having written him (cited from Arntz, *Handbuch der Runenkunde*, 2nd ed. [Halle, 1944], p. 249). If we allow this to mean that writing letters in runes on wood was a common practice, we have evidence from ca. 600 A.D.

<sup>23</sup> The earlier writing systems did not use alphabetical signs, but merely diacritics, for vowels. The Greeks initiated the system of writing vowels, using for this purpose the Phoenecian signs for "weak" consonants, which were not needed for Greek (cf. *ibid.*, p. 17). The possibility of another origin for the vowel signs, mentioned by Arntz, seems slim.

<sup>24</sup> A prime example of this error may be seen in Jellinek's discussion of the alphabet, especially p. 28ff. The origin of the Gothic alphabet is of interest here only in that it affords evidence of Gothic pronunciation.

<sup>25</sup> I do not believe that we can deny the arbitrariness of the linguistic sign. Most of the objections to de Saussure's "signe linguistique" (Cf. de Saussure, *Cours de linguistique générale* [Paris, 1916], pp. 99-113) are terminological rather than basic (Cf. Niels Ege, "Le signe linguistique est arbitraire", *TCLC*, V (1949), pp. 11-29, for a discussion of the problem).

taken to mean that we are not privileged to assume that the borrower of a sign system will use each sign in the value which is closest to the value it had in the language it was previously applied to; but the sounds of two languages are seldom exactly alike. Let us take a hypothetical language in which phonemic opposition obtains between [p] [b] [f]; let us suppose that signs are borrowed from another language which has the opposition [p] [b̥] [f]. Would not then the sign for [b], *ceteris paribus*, be used in the value [b̥]? Even this speculation is invalid, however, for who can tell what another person will do? Wulfila could have used some or all of the signs in a completely new value.<sup>26</sup> This has been seriously proposed, for insufficient reason as we shall later see, for the sign  $\phi$ . We cannot be sure of this until we examine other evidence as to the pronunciation of these signs. To do this is not to use a circular approach as discussed in the preceding paragraph, since we will have determined the origin of the signs first, before consulting other evidence. The alphabet material can only tell us the sound type and the relative position of a sign, never its absolute value. For these reasons, it was thought best not to discuss the pronunciation of the signs in this chapter, but to wait until the end of Chapter II, (sect. 2.3) when we will have discussed the loanwords. The phonetic value of the signs from the point of view of the alphabet and the loanwords is discussed there.

### 1.1 THE ORIGIN OF THE SINGLE SIGNS

#### 1.11 *Signs certainly borrowed from the Greek*

The following signs are almost certainly of Greek origin, and are so considered by all competent judges:  $\alpha$  (A),  $\beta$  (B),  $\Gamma$  (Γ),  $\delta$  (Δ),  $\epsilon$  (E),  $\zeta$  (Z),  $\iota$  (I),  $\kappa$  (K),  $\lambda$  (Λ),  $\mu$  (M),  $\nu$  (N),  $\pi$  (Π),  $\tau$  (T),  $\upsilon$  (Υ),  $\chi$  (X),  $\psi$  (Ψ),  $\phi$  (Φ). Without going more deeply into the variants of all these signs (i.e. the variant  $\bar{\alpha}$  of  $\alpha$  and why the lower bar was lost), we may accept this conclusion, noting that  $\delta$ ,  $\epsilon$ ,  $\zeta$ ,  $\iota$ ,  $\kappa$ ,  $\mu$ ,  $\nu$ ,  $\tau$  and perhaps  $\beta$  could be also of Latin origin, but that the Greek is preferred because of the order in which these signs are written, which is obviously Greek — their numerical value agrees with the Greek.<sup>27</sup> It is to the other signs that we must direct our attention here.

#### 1.12 $\alpha$

The sign  $\alpha$  (using always the form of scribe II in CA) has the following basic variants:

<sup>26</sup> Of course it is unusual that a person will disregard the value of signs borrowed. One of the few cases of this type on record is that of Sequoyah, who used English signs in entirely different values. The fact that none of the values correspond has led some to believe that Sequoyah deliberately disregarded the English values (cf. D. Diringer, *The Alphabet* [New York, N.D.] p. 176f.), thus actually taking them into account.

<sup>27</sup> It is perhaps worth noting here that Ullmann (cited Joan Blomfield, "Runes and the Gothic Alphabet", *Saga Book of the Viking Society*, XII [1941-1942], p. 210) believes that, when an alphabet is taken over in its original order, it proves that it is from an abecedarium accompanied by oral sounding of the names. A use of this criterion might clear up some points of pronunciation, but cannot be used (as it is by Blomfield) to discuss the origin of the alphabet.

CA  $\alpha$ , Carol.  $\vartheta$ , Ambr. A  $\mathfrak{w}$ , Ambr. E  $\mathfrak{a}$  and  $\mathfrak{q}$ . We note that in the Gothic alphabet as exhibited in both hands, curving lines commonly do not close at the top (cf.  $\mathfrak{x}$ ,  $\mathfrak{z}$ ,  $\mathfrak{o}$ ,  $\mathfrak{s}$ ,  $\mathfrak{c}$ , etc.) because of the exigencies of the straight pen. No sign of whose origin we are certain, and which had originally a closure at the top, has a closure in the Gothic alphabet. If we observe the mss. in which both  $\mathfrak{u}$  and  $\mathfrak{a}$  occur, we find them differentiated in the following manner: CA by the curve of the  $\mathfrak{a}$  as opposed to the straight line of the  $\mathfrak{u}$ , and by the tail or descender on  $\mathfrak{u}$ ; in Ambr. E of the Skei-reins in the same manner, except that the back of the  $\mathfrak{a}$  (i.e.  $\mathfrak{a}$ ) is curved. In the Salzburg-Vienna manuscript by a variation in form ( $\mathfrak{uu}$ ) and by the tail ( $\mathfrak{y}$ ). The distinguishing mark is then seen to be the tail and the curvature. If we are justified in reconstructing a form with closure at the top, it is quite probable that it originally had no descender, for we could not easily explain the loss of the descender, as the curvature of the left stroke would have been distinguishing mark enough, and there would have been no need to drop the tail in order to distinguish this form from  $\mathfrak{u}$ . We could not otherwise explain the curvature, for the tail would distinguish  $\mathfrak{u}$  amply from a form  $\mathfrak{a}$ , such as suggested by Jellinek.<sup>28</sup> We then reconstruct  $\mathfrak{a}$  as the prototype. Upon examining the Greek and Latin alphabets of the 4th century, and the various runic alphabets, we find only the Latin  $\mathfrak{a}$ ,  $\mathfrak{q}$ ,  $\mathfrak{r}$  resembling it, the last of which is identical with the form found in the Vatican codex, except for the closure at the top. To explain the lack of a tail in the other manuscripts, we note that a small tail would have been absorbed by the rather large right-hand finish in these manuscripts.<sup>29</sup> A discussion of the pronunciation of this sign and the reasons for the protracted discussion of it are found in section 2.32 g.

### 1.13 $\mathfrak{h}$

$\mathfrak{h}$  is doubtless from Latin, and we need not ponder here the question as to why Wulfila did not use the *spiritus asper*.<sup>30</sup>

### 1.14 $\mathfrak{f}$

The origin of the sign  $\mathfrak{f}$  has bothered scholars since the inception of Gothic studies.<sup>31</sup>

<sup>28</sup> Cf. Jellinek (following Wimmer), p. 25.

<sup>29</sup> On the finishings of Hand II, cf. Fairbanks and Magoun, p. 317f.

<sup>30</sup> Cf. Bouuaert, p. 426. As is remarked above, *passim*, the traditional treatment, which attempts to find the basic reasons for Wulfila's choice of signs, is methodologically unsound. There may be some reason to believe that  $\mathfrak{h}$  is a post-Wulfilan sign, but such conjecture is of course useless. We must operate with the evidence at hand.

<sup>31</sup> It was originally considered to be from the Greek  $\varphi$ , which Wulfila had merely used in the value [p] (Cf. Arntz, p. 118); both von Friesen and Hermann attempt to derive it from the *thorn*-rune, which, according to von Friesen ( $\neq$  8a) comes from Greek  $\phi$ . According to Hermann, the original form was  $\mathfrak{b}$ , and the left-hand curve is merely "... Anschwung zu dem Bogen rechts (Hermann, p. 138)". It is unfortunate that Hermann failed to note that this idea had already been proposed by W. Grimm and shown to be incorrect by Galelentz and Löbe (p. 14). Bouuaert, who has the latest treatment, believes it to come from a form  $\mathfrak{e}$ , which was straightened up into  $\mathfrak{f}$  by devious processes (Bouuaert, p. 430f.).

This is because of lack of attention to the forms actually attested. There are three basic forms of this letter found in the manuscripts:  $\phi$  (in CA and Ambr. A and C),  $\psi$  (in the deeds and the Salzburg-Vienna manuscript),  $\mathfrak{A}$  (in the deeds and the marginal notes to Ambr. A). Two of these symbols have the vertical line curved at the bottom, whereas one does not. The form  $\phi$  also occasionally has a variant  $\phi$ ; the form  $\psi$  has the variants  $\mathfrak{A}$  (in the hand of Wiljarip and in the Salzburg-Vienna manuscript) and  $\mathfrak{A}$  (in the Arezzo deed).  $\psi$  could not be derived from  $\mathfrak{A}$  unless we assumed a reversal of the direction of the curve, for the curve touches on the right in  $\mathfrak{A}$ , but on the left in  $\psi$ . If we assume secondary influence of the Greek,<sup>32</sup> we may derive  $\mathfrak{A}$  from  $\phi$ .  $\phi$  could be derived from  $\psi$ , but  $\psi$  could not be derived from  $\phi$ ; for if the touching of the line on the left were to be considered merely an effect of the scribe's haste, we should expect to see the curve occasionally touch on the right, which it seems never to do. We must then assume a prototype  $\psi$  or  $\mathfrak{A}$  (since this latter form is also attested, and commonly all vertical descenders are also ascenders). Comparing this with the other alphabets, we find the form  $\mathfrak{A}$  in the Greek of the fourth century, and assume this to have been the prototype.<sup>33</sup>

### 1.15 $\varsigma$

Scholars have proposed many possible prototypes for this sign.<sup>34</sup> None of these, however, satisfies our requirement (sect. 1.0) that we may be able to derive all the variants from the prototype suggested; all neglect some of the variants. It is necessary to assume some secondary influence to be able to derive all the variants from one prototype. Since this is true, we may reconstruct various prototypes. Those who believe the prototype to be Latin  $\varsigma$  must explain the bar of some forms ( $\varsigma$ ,  $\epsilon$ ) as coming from secondary influence of the Greek or as the generalization of an accidental variant ( $\epsilon$ ) of  $\varsigma$ , caused by its resemblance to the  $\epsilon$ -form.<sup>35</sup> Those who believe Greek  $\epsilon$ , to be the prototype must explain the loss of the bar ( $\varsigma$ ,  $\epsilon$ ,  $\varsigma$ ) by secondary influence of Latin  $\varsigma$ , or assume that it was lost because it was an unnecessary distinguishing mark. A prototype not seriously proposed as yet is Greek  $\epsilon$ . The reason for the neglect of this form as the prototype is obvious. The usual interpretation is based on the presupposed sound value of the sign. This form, which is the capital

<sup>32</sup> Since  $\mathfrak{A}$  is a cursive form of Greek  $\phi$ , we may easily understand why a scribe, fooled by the resemblance, might replace  $\psi$  by this form in his haste. We need only to presuppose a knowledge of the Greek writing system on the part of the scribe, and this, it seems, is not too daring a supposition for a professional scribe.

<sup>33</sup> Bouuaert also arrived at this conclusion, but by different means, cf. *supra*, p. 15, note 4. On the form  $\mathfrak{A}$ , see Thompson, p. 193.

<sup>34</sup> Von Friesen, # 6, believes it to derive from runic ( $\mathfrak{A}$ ) or Greek ( $\epsilon$ ). Bouuaert (plate 1) agrees that it derives from runic. This viewpoint is not original with von Friesen, but goes back to Zacher (cited by Weingaertner, p. 47). Gabelentz and Löbe (literature cited on p. 15) believed the prototype to be Latin  $\varsigma$ .

<sup>35</sup> Such accidental variants are not uncommon in the hand of Scribe II of CA (see, for example, Mc. 7,7). I do not have exact figures, but such variants are found at least ten times in the hand of Scribe II. These variants may have been caused by the scribe being familiar with an  $\epsilon$ -like form.

form, also has the advantage of having the same numerical value (i.e. 60) as the Gothic sign. There are thus four possible prototypes: runic  $\mathfrak{g}$ , Greek  $\epsilon_1$  or  $\xi$ , and Latin  $g$ .

1.16  $\mathfrak{n}$ 

This sign does not seem to have significant variants; occasionally the curvature at the top is almost completely left off ( $\mathfrak{n}$  in the marginal notes to Ambr. A, cf. the facsimile in Gabelentz and Löbe). It is doubtless from the runic alphabet.<sup>36</sup>

1.17  $\mathfrak{k}$ 

This form is from the Latin alphabet. The rather small head could be explained if we assumed that Wulfila used Greek  $\rho$ , and that later scribes altered this letter to make it more like the more familiar Latin letter. This conclusion is not mandatory, and is doubtless going too far.

1.18  $\mathfrak{f}$ 

$\mathfrak{f}$  is either from the runic or the Latin alphabet. Von Friesen believed that the ascender forced us to assume at least passage through the runic alphabet.<sup>37</sup> This is not true; in the Gothic alphabet descenders are usually also ascenders.<sup>38</sup>

1.19  $\mathfrak{q}$ 

This sign is derived by most authorities from the rune  $\mathfrak{x}$ .<sup>39</sup> In so doing, however, we must postulate a change of the broken lines, much easier to make with a straight pen, to curved lines. There may be good reason for this, as broken lines are generally avoided in the Gothic script, and smooth transitions are more common. There is, however, no reason to discard Greek  $\alpha$  as its archetype, since  $\alpha$  is not an uncommon variant of  $\omega$ , and since the exigencies of the straight pen may well have mitigated against  $\ast\mathfrak{q}$  (requiring four strokes of the pen) in favor of  $\mathfrak{q}$  (two strokes). If we may

<sup>36</sup> Unless we may assume that this letter's likeness to  $\mathfrak{a}$  (if it comes from Latin  $u$ ) caused Wulfila to change it by turning it over. Such an assumption would relieve those who consider it methodologically better to derive the Gothic alphabet from the least possible number of sources (von Friesen, # 6, Bouuaert, p. 436 f.), but the only reason for such an assumption is a (supposed) pronunciation of the sign.

<sup>37</sup> Von Friesen, # 6: "Auch got.  $f$  trägt Spuren davon, dass es das Runenalphabet passiert hat, bevor es unter die got. Schriftzeichen aufgenommen wurde."

<sup>38</sup> This has led Bouuaert to state (p. 437) that Wulfila used the minuscule principle. This is an anachronism of the grossest sort. To speak of a minuscule writing in the fourth century is possible (cf. Thompson, p. 103, where we see the expression "minuscule, or small, letters"), but the minuscule principle was developed much later!

<sup>39</sup> Indeed, Bouuaert is the only reputable scholar who has disputed this derivation (cf. p. 427), but he ends up by accepting it (p. 437). Earlier scholars (as, for example, Weingaertner, p. 31) assumed a secondary influence of Greek  $\alpha$  ("... ihrer Form nach ist sie dem griech. Omega ähnlich geworden").

trust Gothein, the form  $\mathfrak{A}$  is actually used for  $\mathfrak{Q}$  in the marginal notes of the Veronensis.<sup>40</sup> This derivation has the added advantage that the numerical values correspond. We have thus two possible sources, runic  $\mathfrak{A}$  and Greek  $\alpha$ .

### 1.20 *Excursus on the sign $\Theta$*

The origin of this sign cannot be determined with any certainty. It cannot therefore be used to determine the pronunciation of Gothic and properly does not belong in this chapter. It is important that we discuss it here, however, since the usual interpretation of its prototype would make our derivation of  $\Phi$  suspect. It further points up problems of methodology.

It is commonly assumed that  $\Theta$  is merely the uncial form of the Greek *theta*, which Wulfila used in a new value.<sup>41</sup> Wulfila would thus have used the sign for *theta* in two values.<sup>42</sup> The usual interpretation, however, neglects the variants, taking the form of scribe II of CA as the point of departure.<sup>43</sup> Any proposal as to the prototype must take into account the variants  $\mathfrak{u}$ ,  $\mathfrak{v}$ ,  $\mathfrak{e}^1$ ,  $\mathfrak{e}$ ,  $\mathfrak{u}$ ,  $\mathfrak{e}$ ,  $\mathfrak{u}$ ,  $\mathfrak{e}^1$ . I do not believe that these could all be derived from a prototype  $\mathfrak{e}$ . Bouuaert believes this sign to be a Greek  $\omicron$  (to indicate lip-rounding and back articulation) and the sign  $\cdot$  (*spiritus asper* ?) (to indicate the aspiration).<sup>44</sup> In spite of the fact that this may seem to be crediting Wulfila with too much linguistic acuteness, this may not be too far from the truth. If Durnovo is right, the "spiderlike x" of the glagolitic alphabet signified a labio-velar spirant.<sup>45</sup> This sign ( $\mathfrak{x}$ ) is merely an  $\mathfrak{o}$  with diacritic marks (perhaps an *H* ?) around it.<sup>46</sup> We do not know where this sign came from, but perhaps the Gothic sign may be of the same or of analogous origin. If Wulfila has here created a new sign, it is the only new creation in his alphabet. At any rate,  $\mathfrak{e}$  cannot be proposed by any serious scholar as the prototype.

<sup>40</sup> Percy Gothein, "Zu den Gotica Veronensia", *ZfdA*, LXVII (1930), p. 207. It seems incredible that we have no trustworthy reading of this manuscript by a competent Gothic specialist, as Gothein (admittedly no specialist) noted several errors in reading by Capelle, who actually had not seen the manuscript he was reporting on! It should be noted that the Veronese notes are probably the oldest extant Gothic fragments (cf. D. B. Capelle, "Les homélies 'De lectionibus euangeliorum' de Maximin l'Arien", *Revue Benedictine*, XL [1928], p. 49).

<sup>41</sup> Von Friesen, "Got. Schrift", # 6; L. F. A. Wimmer, *Die Runenschrift* (Berlin, 1887), p. 259 ff. Jellinek (p. 26 f.) holds out the possibility of a runic origin, and Hermann (p. 137) states this unequivocally.

<sup>42</sup> To speak of two values is not to use a circular argument, since I have said we cannot use this sign in determining Gothic pronunciation. The difference between the signs is obviously significant in the two forms  $\Phi$   $\mathfrak{Q}$  and  $\Theta$   $\mathfrak{Q}$ , though free variation occurs in "graphemics" also.

<sup>43</sup> Von Friesen's list of variants unfortunately contains no variants of  $\Theta$ , nor is Bouuaert much better in this respect. Of course, Gabelentz and Löbe list almost all the variants.

<sup>44</sup> Bouuaert, p. 434.

<sup>45</sup> *Byzantinoslavica*, I (1929), p. 71 f.

<sup>46</sup> Trubetzkoy, "Die Aussprache des griech.  $\chi$  im 9. Jahrhundert n. Christus", *Glotta*, XXV, 248-256, believes that this sign comes from Greek  $\chi$ , but I cannot understand his reasoning.



## LOAN WORDS AND TRANSCRIPTIONS OF PROPER NAMES

### 2.0 METHODOLOGICAL REMARKS

The loan words are usually broken up into three categories: (1) the transcription of Biblical names in Gothic, (2) the transcription of Gothic words and names by classical authors, (3) the treatment of loans from Gothic into other languages, chiefly Romance.<sup>1</sup> Only the first is discussed here, and for the following reasons. In order to use the transcription of Gothic words and names by classical authors, we must assume that the Gothic represented in these “had the same phonological structure as biblical Gothic”.<sup>2</sup> To make such an assumption is to ignore the findings of dialect geographers. Criteria (2) and (3) must be relegated to comparative evidence. We must first determine what such transcriptions and loans can tell us for the dialect from which they are borrowed, and then compare this dialect with Wulfila’s. The judgement of Jellinek is eminently sensible:<sup>3</sup>

Es ist auch ohne weiteres zuzugeben, dass die meisten argumente, die man in den handbüchern findet, für Wulfilas sprache nichts beweisen. Weder das *hailag* des rings von Pietroassa, das auch Marstrander scrupeln macht, denn nicht Wulfila hat die Runen geritzt; noch das provenzalische *raus*, denn nicht aus Wulfilas mund haben die Romanen Südgaliliens das got. wort gehört.

### 2.1 TRANSCRIPTION OF BIBLICAL NAMES

#### 2.10 *Methodological remarks*

There are about 220 biblical names in our Gothic material. They are probably the most reliable source we have for determining the phonetic value of Gothic signs, and they have received extensive treatment almost from the inception of Gothic studies.<sup>4</sup>

<sup>1</sup> Cf. Jellinek, p. 30; Braune-Helm, p. 4; Streitberg, p. 53f.

<sup>2</sup> Cf. Wm. G. Moulton, “The Stops and Spirants of Early Germanic”, *Language*, XXX (1954), footnote 14.

<sup>3</sup> M. H. Jellinek, “Gotica”, *ZfdA*, LXVI (1929), p. 117f.

<sup>4</sup> The first treatment of them which attempted to be at all complete was by W. Weingärtner, *Die Aussprache des Gotischen zur Zeit seines Entstehens*, (Leipzig, 1858). Cf. also W. Ebel, “Die Fremdwörter bei Ulfilas in phonetischer Hinsicht”, *ZvS*, IV (1855), pp. 282-288; C. Elis, *Ueber die Fremdwörter und fremden Eigennamen in der got. Bibel* (Göttingen, Dissertation, 1903); W. Schulze, “Gotica”, *ZvS*, XLI (1909), pp. 165-175. The best treatment to date, with very trustworthy word counts and excellent methodological discussion, is that of K. Gaebeler, “Die griechischen Bestandteile der goti-

Yet, in spite of the large volume of material written on the subject, and the assurance evinced in some quarters, we must admit that previous treatments have failed by virtue of errors in conception. The problem is both more and less complicated than heretofore considered. We do not know what the Greek original from which the Gothic was translated was like. The Gothic version is the oldest extant representative of the Antiochian or Lucianic type of text, and it would seem fairly easy to reconstruct the *Vorlage*,<sup>5</sup> but this has not been done with any satisfactory results. The attempt by Streitberg, usually considered the most authoritative,<sup>6</sup> must be discarded on methodological grounds and because of his lack of care in choosing his sources.<sup>7</sup> It is uncritical use of Streitberg and a lack of sophistication in matters of NT criticism which mar much of the fine work of Gaebeler.<sup>8</sup> We must also consider the following points in our attempts to make use of these names: 1. the accents and breathings of proper names cannot be fixed for Wulfila's time with any authority, for it is rare that these are indicated in NT manuscripts before the 7th century;<sup>9</sup> 2. in matters of spelling

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schen Bibel", *ZfdPh*, XLIII (1911), pp. 1-117. All numbers given in the following discussions are my own, however, unless the source is expressly stated.

<sup>5</sup> On the Gothic version as the oldest representative of the Antiochian text, cf. B. M. Metzger, "Evidence of the Versions for the Text of the NT", *NT Manuscript Studies* (Chicago, 1950), p. 62 f. and André Wilmart, "Les évangiles gothiques", *Revue Biblique*, XXXVI (1927), p. 51. It seems that an interlinear of the Gothic Bible could tell us much about the *Vorlage* (A. Jülicher, "Die griech. Vorlage der got. Bibel", *ZfdA*, LII (1910), p. 365f. and M. Metten, "What a Greek interlinear of the Gothic Bible Text can teach us", *JEGPh*, 1933, pp. 530-548), but one has not been made.

<sup>6</sup> Almost all textual studies since 1920 use Streitberg as their authority, accepting his Greek *Vorlage* as fact, and he is everywhere cited as *the* authority. Voices are being heard against this uncritical attitude. See particularly Metten, and Friedrichsen's two books (p. 00, note 6, *infra*), where Streitberg is still used to a great extent, perhaps because we have no source to replace him. In my opinion, it would be better to go back to Bernhardt.

<sup>7</sup> Cf. Metzger (note 5, above), p. 63, footnote 297. Streitberg is wrong when he maintains: "Ebenso steht es von vornherein fest, dass der Uebersetzer den Text zugrunde gelegt haben *muss* [italics mine], der in seiner Diözese ... geherrscht hat ..." (p. XXXI, *Got. Bibel*). We do not know what Wulfila *must* have done, and B. H. Streeter (*The Four Gospels* [London, 1924], pp. 113ff.), gives good reason for assuming that Wulfila used the text current in Antioch. But his main error is in choosing von Soden as his authority for NT criticism (cf. *Got. Bibel*, XXXVII-XL). It is exactly that point upon which Streitberg bases much of his theory (the subgroups of the I- and K-text) at which von Soden breaks down; F. Kenyon says (*Our Bible and the Ancient Manuscripts* [New York, 1948], p. 118): "Von Soden claimed this family (I) as his special discovery ..., but in truth it is difficult to identify, and consists of a number of incongruous groups. The identification of the Caesarean family, which forms a part of it, has further discredited it ..." I know of no authority save Moffat (he used von Soden's text as a model for his Bible translation) who accepts von Soden's textual theory; it has been expressly rejected by such outstanding authorities as Robertson, Kenyon, Lake, Souter, and Turner (cited Kenyon, *loc. cit.*).

<sup>8</sup> On his use of Streitberg, cf. Gaebeler, p. 9. If one wished to do as Gaebeler has done, and consult Tischendorf's *Octava maior* with the purpose in mind of finding a correspondent for a certain Gothic form, one could, no doubt, find evidence of some Greek manuscript for almost every form. *Loc. cit.*, for example, he cites as evidence Chr., U, E, V, D (assume his Be. indicates the *Codex Bezae*). As any textual critic knows, this is not sound procedure. Especially is the use of the *Codex Bezae* to be regarded as methodologically unsound, since this manuscript is known to contain many curious readings, and since its text is disputed by such authorities as Kenyon and Souter. At any rate, it is doubtful that Wulfila could have used a collation of all these manuscripts.

<sup>9</sup> Westcott and Hort, *The NT in the Original Greek* (New York, 1944), p. 580.

it is not uncommon to find the mss. of the Greek differing among themselves, even those coming from the same scriptorium — it is a well-known fact, for example, that Aleph prefers ι to ει, while B is fond of ει and not ι (Tischendorf and Keñyon believe the scribe of Aleph and of B to be from the same scriptorium).<sup>10</sup> Because of these facts, arguments concerning *hapax legomena* are fruitless; the argument of Gaebeler and Luft, for example, as to whether Gothic *Lima* is the transcription of λημα or λιμα.<sup>11</sup> The only sensible attitude to take in the face of these facts is to base our case on the usual correspondences and to discard isolated instances as evidence. Thus, if ε is rendered by ʀ 83 times, it is really unnecessary to note that it is rendered 3 times by e and 3 times by ι

It is further to be noted that we must consider the characteristics of that part of our text in which the form is found.<sup>12</sup> We should suspect the reliability of Mark and Luke, especially when “errors” there parallel those in Gothic forms. As the deeds and the Calendar are known to be non-Wulfilian, they should be excluded from discussion at this point. The OT fragments are also probably non-Wulfilian,<sup>13</sup> and their witness should be used with caution. It is to be understood that I have taken all these reservations under consideration in the discussion of these Biblical names.

2.10.1 We must reiterate the same word of caution given in the case of alphabet correspondences (section 1.03 above). Even if we succeed in solving in every detail Wulfila’s transcriptions of Greek proper names, we cannot learn from this the exact phonetic value of Wulfila’s signs. We can only learn the relative positions of the sounds. Even this is not always possible. To some American ears, the long monophthongal [e] of German sounds like [ε]. We must view the transcriptions from the point of view of the system, we must not treat them in isolation.

## 2.11 *The Essential Correspondences*

### 2.11.1 *The Consonants*

π, τ, κ = 𐍑 𐍒 𐍋 cf. M 26, 29, *Paitrus* (Πέτρος); K 16,10, *Teimaupaius* (Τιμόθεος); J 18,14, *Kajafa* (Καϊάφας).

β, δ, γ = 𐍂 𐍄 𐍇 cf. M 10, 25, *Baiailzaibul* (Βεῖλλζεβουλ); R 16,23, *Gaius* (Γάϊος); J 7,42, *Daweid* (Δάβειδ).

γγ = 𐍆𐍆 cf. L 3,25, *Naggais* (Ναγγαί).

φ, θ, χ = 𐍑 𐍓 𐍅 cf. J 6,7, *Filippus* (Φίλιππος); *Teimaupaius* (Τιμόθεος); L 8,3, *Kusins* (Χουζῖν).

σ, ζ = 𐍍 𐍎 cf. R 9,29, *Sabaoh* (Σαβαώθ); L 1,5, *Zakarias* (Ζαχαρίας).

<sup>10</sup> A. T. Robertson and W. H. Davis, *A New Short Grammar of the Greek Testament* (New York, 1933), p. 31.

<sup>11</sup> Gaebeler, p. 9.

<sup>12</sup> Many of the following points are noted also by Gaebeler (*ibid.*, pp. 1 ff.), but he neglects them in his discussion.

<sup>13</sup> This is the usual opinion since Ohrloff, “Die alttestamentlichen Bruchstücke der Got. Bibel-übersetzung”, *ZfdPh*, VII (1876), pp. 251-295.

λ, ρ = 𐌺 𐌹 cf. C 4,14, *Lukas* (Λουκάς); Mc. 15,21, *Rufus* (Ροῦφος).  
 μ, ν = 𐌿 𐌽 cf. M 27,56, *Magdalene* (Μαγδαληνή); L 18,37, *Nazoraius* (Ναζωραῖος).

σ = 𐌸 before voiced consonants in 2 Cor. 1,13: *Mosez lagida*, in Lc. 3, 25: *Aizlemis* (τοῦ Ἑσλῑμ) and in *prezbyt-* (πρεσβυτ-), but not in the OT fragments, cf. Neh. 7, 17.29: *Asgadis*, *Asmopis* (Ἀσγαδ, Ἀσμωνθ).<sup>14</sup>

χ = 𐌷 9 times, and 𐌶 18 times.

We find occasionally that Greek φ, θ, σ are rendered by Gothic 𐌶, 𐌵, 𐌸, respectively, in medial position. Examples are found throughout the material, cf. Skeireins 2,2 *Ioseba* (Gk. Ἰωσήφ); Lc. 3,38 *Sedis* (Σήθ); Lc. 3,3 *Faraizis* (Φάρεις).

Greek κου = Gothic 𐌵 in *Qartus* (Κουάρτος) R 16,23.

The *spiritus asper* is sometimes rendered by 𐌺, sometimes not rendered at all, cf. *Helias* (Ἡλίας), *Haibraius* (Ἑβραῖος), but *Iairaupaulin*, (Ἱεροπόλει), *Iairusalem* (Ἱεροῦσαλημ). Because of this, Luft and Gaebeler have come to the conclusion that Wulfila did not use a sign to render the *spiritus asper*, but that later scribes added some 𐌺 's under Latin influence.<sup>15</sup> If this is true, it is strange that we do not find \**Hiairusalem*, since *Hierusalem* is the common form in Old Latin mss.<sup>16</sup>

### 2.11.2 The Vowels

α = 𐌺, cf. G 4,24 *Agar* (Ἄγαρ).

ε = 𐌶, cf. L 3,3 *Faraizis* (Φάρεις).

η = 𐌶, cf. Lc. 3,38 *Sedis* (Σήθ).

αι = 𐌶, cf. M 27,57 *Areimaphaias* (Ἀριμαθαία).

ω = 𐌶, cf. R 9,29 *Sabaoh* (Σαβαώθ).

ο = 𐌶, cf. K 16,10 *Teimaupaius* (Τιμόθεος).

ει = 𐌶, before vowels, cf. G 2,11 B *Antiokjai* (Ἀντιόχεια).

ι = 𐌶, in closed syllables, cf. J 6,7 *Filippus* (Φίλιππος), ει in open syllables over 40 times, cf. J 6,68 *Seimon* (Σίμων). 𐌶 ca. 20 times, almost only in *hapax legomena*,<sup>17</sup> cf. Mc. 15,34 *Sibakpane* (Σιβακθανει).

ι = both ει and 𐌶 in antevocalic position, cf. Mc. 15,22 *Jaeirus*, Lc. 8,4 *Iaeirus* (Ἰάειρος).

ου = 𐌶, cf. L 8,3 *Kusins* (Χουζῑ).

υ = 𐌶, cf. E 6,21 *Twkeikus* (Τύχικος).

οι = 𐌶, cf. Mc. 7,26 *fwnikiska* (Φοινίκισσα).

<sup>14</sup> It is to be noted that, except for some rather surprising changes, the OT fragments usually keep closer to the Greek, letter for letter.

<sup>15</sup> W. Luft, "Die Umschreibungen der fremden Namen bei Wulfila", *ZvS*, XXV (1897), 310 f.; Gaebeler, p. 16f.

<sup>16</sup> Cf. also W. Schulze, "Griechische Lehnworte im Gotischen", *Sitzungsberichte der ... preussischen Akademie*, 1905, p. 746f.

<sup>17</sup> These are Gaebeler's figures (p. 22f.); my figures (in which *ei* in the combination *aei* is not counted) have 𐌶 = *ei* in open syllables 36 times.

2.11.21 *ai*. — All previous discussions have considered the digraph under each separate symbol, so that *i* is placed with the cases of *i* in open syllable. That such a treatment does violence to the facts is seen by the rendering of Greek Ἀχαῖα, Ἀχαῖας in Gothic (*Akaja*, *Akajus*); if each symbol represented a single vowel, we should expect accented *i* to be rendered by either *ei* or *i*. It is clear that the digraph *ai* represented a diphthong in Greek, unless there has been a change of accent, which would not be explainable. The following is a count of the individual occurrences (not of lexical items): before vowel: *ai* = *aj* 4 times, *aij* 2 times; *ai* 10 times; *aei* 5 times; *aw* occurs for *ai* in *Babawis* for (Neh. 7,16); before consonant: *aei* 6 times; *ai* 2 times.<sup>18</sup>

## 2.2 THE PRONUNCIATION OF 4TH CENTURY GREEK

### 2.20 Methodological remarks

I have decided to use Sturtevant's *Pronunciation of Greek and Latin* as a basis for my discussion of Greek pronunciation in the 4th century, since I am admittedly unfamiliar with this field.<sup>19</sup> Yet it is oftentimes a fatal error to accept secondhand information with an uncritical eye, and one is often prone to choose one's source in such a manner as to receive the most support for one's pet theories.<sup>20</sup> It is then occasionally necessary for me to take exception to a statement by Sturtevant, and reasons are given for this in all cases. I must, for example, object to Sturtevant's "particularly cogent proof" for the open value of Greek η,<sup>21</sup> in which he argues that the use of βῆ βῆ to render the cry of a sheep shows that η must have been either [α], [a], or [ε], but that the cry of a sheep cannot be rendered by [be be]. This is, however, a common rendering of the sheep's cry in modern French. One finds Sturtevant to be somewhat uncritical in his choice of sources (he uses Luft as his authority on Gothic loanword treatment) and in his arguments from other languages. Thus, when he says that the fact that Gothic ƿ is pronounced [w] shows that Greek υ was pronounced [w] in Wulfila's time,<sup>22</sup> he neglects to note that our only real evidence for the Gothic pronunciation being [w] is Greek. We may not use such a circular argument to establish absolute pronunciation. We may say that, if Wulfila renders two Greek signs by two Gothic signs, this indicates that, to his ear,

<sup>18</sup> It should be noted that W. Schulze believes that ει after a vowel in Greek is a sign of diaeresis (cf. W. Schulze, "Samstag", *ZvS*, XXXIII [1893], pp. 366-386, reprinted in *Kleine Schriften* [Berlin, 1933], p. 290, note 6).

<sup>19</sup> I use the second edition (Philadelphia, 1940).

<sup>20</sup> Thus Jellinek, when he discusses the idea (expressed by Streitberg and others) that Wulfila's alphabet differentiated only as to quality and not as to quantity, accepts Hatzidakis as an authority to show that direct witnesses to the loss of length differentiation in Greek are found only in very late times, in spite of the fact that the whole of scholarly literature is against Hatzidakis on this point.

<sup>21</sup> Sturtevant, p. 34.

<sup>22</sup> *Ibid.*, p. 55.

there was a difference in the sound value of the two symbols in Greek.<sup>23</sup> This is not a certain proof, however, for Wulfila may have chosen arbitrarily *qr* according to the rules of the grammarians, if he knew these rules (Cf. 1.03).

## 2.21 Discussion of the single signs

*α* represented an open low central or front unrounded vowel.

*ι* represented an open high front unrounded vowel.

*η* represented an *e*-like sound, probably a close mid front unrounded vowel, for it has this value in the Armenian rendering of Greek loans.

*η* = *e* 25 times, *ē* 3 times, and *i* 6 times in Armenian renderings of Greek loans,<sup>24</sup> and this value is supported by the language of the catacomb Jews.

The Latin borrowings *amurca*, *purpura*, *ampura*, unless they are borrowed from Etruscan or another Mediterranean language, indicate that *o* represented a close mid back rounded vowel and *ω* still represented an open mid back or close low back rounded vowel down into Hellenistic times.<sup>25</sup> The treatment of Greek loans in Armenian and Hebrew show that *o* and *ω* had coalesced in the Orient by the 5th century. However, the use of Latin *o* to render *o*, and *ō* to render *ω* among the Jews in the catacombs shows that *ω* was [o] and *o* was [ɔ] among these Jews in the 3rd century.<sup>26</sup>

*ou* represented a high back rounded vowel.

*αι* represented an open mid front unrounded vowel.

*ε* represented an open mid front unrounded vowel also.

*οι* represented a high front rounded vowel.

*αυ*, *ευ* represented perhaps a sequence of [a], [ε] plus a high back rounded non-syllabic ([a w], [ε w]), but Sturtevant's use of Gothic as proof of this is not acceptable.<sup>27</sup> We may only infer that Gothic *Υ* = [w] from etymological considerations; it may well have been [ɥ].<sup>28</sup>

*ρ* represented a trilled or flapped apical vibrant.

*λ* represented an alveolar *or*, in some dialects, a retroflex ("dark") lateral. In

<sup>23</sup> *Ibid.*, p. 37.

<sup>24</sup> H. J. Leon, "The Language of the Greek Inscriptions from the Jewish Catacombs of Rome", *Transactions of the American Philological Association*, 1927, 210-233, p. 223.

<sup>25</sup> Sturtevant, p. 47.

<sup>26</sup> Leon, p. 217.

<sup>27</sup> The only indication of the pronunciation of Gothic *Υ* we have is from the Greek; etymology is useless here (cf. footnote directly below), and the internal phenomena are equally invalid in determining the exact pronunciation of a sign. Greek *υ* is usually supposed to have been a high front rounded vowel. If we assume that *υ* had the value of a high front rounded non-syllabic in combination with *α* and *ε*, would not the Goth have used it for his native [w], if he possessed this sound? Thus, American children hear the [ɥ] in French *huit* as [w] and use this sound to render French [ɥ].

<sup>28</sup> Cf. Jellinek, "Gotisch *w*", *ZfdA*, XXXVI (1893), p. 268: "Es wird durch sie (interchange of *u* and *w*) nur bewiesen, dass einmal auch im got. *w* *u*-consonans war, ob es diese geltung auch zur zeit des Ulfilas noch hatte, bleibt unsicher".

Armenian, we find Greek λ rendered by both *l* and *l'*.<sup>29</sup> This may well be an Armenian phenomenon, however.

μ, ν represented bilabial and apical nasals, respectively.

σ represented a voiceless alveolar sibilant in most positions, but it was voiced before voiced consonants.

φ and θ were voiceless spirants, with bilabial and either interdental or alveolar points of articulation respectively, as is indicated by a Byzantine grammarian's description of them,<sup>30</sup> by the rendering of φ by Latin *f* among the Jews in the catacombs at Rome. The fact that φ is rendered by *p'* and θ by *t'* in Armenian is of no value, as this language possesses no voiceless labial or dental spirants.<sup>31</sup> χ seems still to have been an aspirate ([kh]) in the 5th century, as is seen by its being rendered in Armenian by *k'*. Since Armenian possessed a sound [χ], Greek χ must not have been a spirant.<sup>32</sup> The fact that χ was rendered by κ in Gothic may also be taken as an indication that it was not a spirant, except that Gothic seems not to have possessed a velar spirant, except perhaps in certain environments.<sup>33</sup>

π, τ, κ represented voiceless bilabial, alveolar and velar stops, respectively.

β, δ, γ represented voiced bilabial, alveolar and velar spirants, respectively.

γγ and γκ were digraphs used to represent voiced and voiceless velar stops, respectively, preceded by a homorganic nasal.

ζ represented a voiced alveolar sibilant.

∘ represented aspiration.

ξ represented an aspirated velar stop plus an alveolar spirant according to Sturtevant (p. 91). He bases his argument on the fact that it is rendered in Greek loans into Armenian by *k's*. He does not consider the evidence of some inscriptions (admittedly of an earlier date), such as the old boustrophedon inscription of Naxos, where ξ is rendered by 𐀓S.<sup>34</sup> These show that there was an earlier dialect form with a pre-aspirated [s]. This form may have existed in some fourth century dialects.

## 2.2 A. *The pronunciation of certain Latin signs in the 4th century*

This section includes only those signs which are of value in determining the pronunciation of Gothic.

*h* represented aspiration, or zero.

*f* represented a voiceless labio-dental spirant.

<sup>29</sup> Sturtevant, p. 63, following Thumb, *Byzantinische Zeitschrift* IX, p. 404f. Sturtevant has not been very careful in this citation, however, for Thumb gives reason to show that this is actually an inner phenomenon of Armenian. It may be that Sturtevant did not think Thumb's argument valid.

<sup>30</sup> Sturtevant, p. 85.

<sup>31</sup> *Ibid.*, p. 82f.

<sup>32</sup> *Ibid.*, p. 82f. I am not exactly sure that this constitutes a proof; what we usually consider to have been [χ] in Armenian may have had an articulation quite different from [χ].

<sup>33</sup> See sect. 4.21a.

<sup>34</sup> F. Blass, *Ueber die Aussprache des Griechischen*, 3rd ed. (Leipzig, 1888), p. 91.

*g* represented a high front unrounded non-syllabic in position before *e* or *i*.

*qu* represented a voiceless labio-velar stop, and there was a strong tradition to the effect that either *q* or *u* was superfluous (they functioned as one in the syllabification, as in *a/qua*); Donatus says that *u* is neither vowel nor consonant after *q*, and Pompeius adds that in *quidem* *u* is merely "pars litterae praecedentis".<sup>35</sup>

## 2.2 B. *The pronunciation of certain runic signs*

We are unfortunately not in any position to determine with any degree of certainty the value of the runes. The only criteria which can be used are etymology and internal evidence. Both of these are often misused in the study of the sound values of the runes. To use "changes" in the sound values of runes to write a history of the language using them, and then to use these histories to determine the value of the runes is to argue in a circle. The values usually given for the runes are derived from their use in the Scandinavian countries. These cannot be proposed for Gothic. The following sound values are only approximate.

ḡ represented a [j]-like sound.

ṇ represented a [u]-like sound.

ṣ represented a [o]-like sound.

## 2.3 PRELIMINARY CONCLUSIONS BASED ON THE ALPHABET AND THE LOANS

It seems expedient to set down some preliminary conclusions as to the pronunciation of Gothic at this time, in the light of the foregoing discussions. This is useful for the following reasons. We can better discuss the value of the internal phenomena when we know something about the pronunciation of the signs under discussion first. We can better evaluate the evidence of each criterion when we set down the conclusions derived from it, without regard to those derived from others.

### 2.31 *Pronunciation of the vowel signs*

#### 2.31. a ṛ

This sign is derived from a form of the Greek *alpha* (1.11), which it also renders in loanwords and Biblical names (2.11.2). The features which distinguished the phoneme represented by *alpha* from all other phonemes of Greek were "lowness" and the fact that it was articulated in a more central position than all other vowels. We cannot be sure which of these features was the primary one in oppositions and which the secondary. We cannot be sure, therefore, whether it was the fact of low tongue

<sup>35</sup> Sturtevant, p. 169f. On the interpretation of *qu* in Latin, see further R. Godel, "Les semi-voyelles en latin", *SL*, VII (1953), p. 98f. It should be noted that *q* alone is used in some inscriptions (Cf. Jellinek, in a review of Bethge, *AfdA*, XXVIII [1902], 24).



position or central tongue position which was the primary feature of the sound represented in Gothic by 𐍲. 𐍲 was presumably a vowel relatively more central and lower than all others.

### 2.31.b 𐍲

This digraph is derived from the Greek digraph αι, which it also renders in Biblical names (2.11.2). The features which distinguished the phoneme represented by αι and ε in Greek were different for each opposition. It was distinguished from the phoneme represented by *alpha* by a more front and a higher articulating position. It was distinguished from the phonemes represented by *omicron* and *omega* by front position of articulation and/or lack of lip-rounding. It shared with the phonemes represented by η, ει, ι the feature of front articulation and lack of lip-rounding, and presumably was distinguished from η only by having a lower articulating position. Of all these features, we cannot decide which were primary and which were secondary. This must also be true of the sound represented by 𐍲 in Gothic. We define the position of the sound represented by 𐍲 as, relatively, higher and more front than the sound represented by 𐍲, more front and less lip-rounded than the sounds represented by 𐍳 and 𐍴, and lower than the sound represented by the sign ε.

### 2.31.c ε

Our discussion of the sound value of ε is complicated by the following factors: although ε derives from the Greek ε (1.11), it is used to render Greek η in Biblical names (2.11.2). Since there is a possibility that the Gothic alphabet may exhibit forms which are non-Wulfilian (1.01), the evidence of Biblical names must be given greater weight where the criteria of alphabet derivation and Biblical names do not agree. There are several ways in which we may explain this apparent discrepancy. It may be explained as due to a grammatical tradition in which ε was still pronounced [ε].<sup>36</sup> We may assume that Wulfila himself used another sign for this *e*-like sound, which was given up for ε in Italy.<sup>37</sup> We may assume that Wulfila used ε in the value that ε had in Latin.

The phoneme represented by η was distinguished from all other phonemes of Greek in a different manner for each opposition. It was distinguished from the phonemes represented by *omega* and *omicron* by a more front position of articulation and by relative lack of lip-rounding. It was distinguished from the phonemes represented by ι and ει by a lower position of articulation and from the phoneme represented by αι and ε by a higher position of articulation. Gothic ε therefore, represented a sound which was relatively higher than that represented by 𐍲; relatively lower

<sup>36</sup> Cf. Leon, p. 215. It is to be noted that Byzantium was notorious for its grammarians, and that Ullmann believes that, when an alphabet is taken over in its original order, this proves it to be from an abcdarium accompanied by oral sounding of the names (cited above, p. 18, footnote 27).

<sup>37</sup> It is commonly assumed that the reason Wulfila did not use η was its resemblance to his 𐍲. It could just as easily be assumed that later scribes replaced his 𐍲 by ε because of this resemblance.

than that represented by **ɪ** ; more lip-spread and/or more front than that represented by **ʊ** and that represented by **ʏ** .

#### 2.31.d **ɪ** and **εɪ**

We cannot be sure whether **ɪ** or **εɪ** represented the closer (i.e. higher) sound, since the Greek evidence is ambiguous. The phonemes represented by **ɪ** and **εɪ** were distinguished from all other phonemes in Greek by having a higher articulating position than all others of the front series; by being relatively more front and/or more lip-spread than those of the back series. The opposition between **ɪ** and **εɪ** was one of close vs. open or *vice versa*. It is quite probable that the following situation obtained in Gothic: the sound represented by **εɪ** was the highest of the front series; it was distinguished from the sounds represented by **ɪ** and **ʊ** by front position and/or lack of lip-rounding. **ɪ** represented a sound lower than that represented by **εɪ** ; higher than that represented by **ε** ; it was distinguished from the sounds represented by **ɪ** and **ʊ** by front position and/or lack of lip-rounding.

#### 2.31.e **ɪ**

This sign derives from runic, and is used to render **ov** in Biblical names. We cannot be even reasonably certain of its value in the runic alphabet, since this value would naturally be the same as the value of the Gothic sign, and we would have to know how Gothic was pronounced to know the value of the runic sign. The phoneme represented by **ov** in Greek was distinguished from all other phonemes of Greek by various features. It shared with the phonemes represented by *omicron* and *omega* the feature of "backness" and lip-rounding. It was articulated higher than either of these. It was distinguished from the phonemes represented by **ɪ** and **εɪ** by back articulation and lip-rounding. It is impossible to determine which of these features was the primary one. The position of the Gothic sound represented by **ɪ** may be defined as follows: it was higher than that represented by **ʊ** ; it was distinguished from that represented by **εɪ** or **ɪ** by back articulation and/or lip-rounding.

#### 2.31.f **ʊ** .

This sign derives either from runic **ʌ** or Greek **ʊ**, and is used to render Greek **ω** in Biblical names. The phoneme represented by Greek **ω** was distinguished from the phoneme represented by **ov** by a lower articulatory position; it was distinguished from the phoneme represented by *omicron* by a higher tongue position. Thus we may define the position of the sound represented by **ʊ** in Gothic as follows: it was articulated in a higher position than that represented by **ʏ** ; it was distinguished from the sounds represented by **ε** and **ʏ** by back position and/or lip-rounding.

#### 2.31.g **ʏ**

The derivation of this digraph is uncertain. It is used to render Greek **o** in Biblical

names. There have been numerous proposals of reasons why Wulfila did not use Greek *omicron* in his alphabet; none of them are acceptable. In using 𐌺𐌽, he may have been influenced by the Latin tradition or by analogy to 𐌿. Its position is pretty well defined by the above considerations of the other vowels. It represented a sound higher and more back than that represented by 𐌿; this sound was lower than that represented by 𐌾; it was distinguished from the sound represented by 𐌿 and the sound represented by 𐌺 by back position and/or lip-rounding.

### 2.31.h 𐌺 𐌺̅ 𐌺̆, 𐌿 𐌿̅, 𐌿̆

These (before vowels) represented sequences of the type low central vowel plus a high (front) non-syllabic (i.e. presumably something like [a<sup>h</sup>], but there is a whole range of possibilities). We may explain the variation in writing by the fact that this sound was not found in native Gothic words. 𐌿̆ can be explained as being merely an imitation of the Greek.

Thus the relative position of the vowel sounds represented by the signs of the Gothic alphabet may be set down graphically as follows:

𐌺̅ 𐌺̆ (2.31.d)	𐌺̅ 𐌺̆ (2.31.c)
𐌺̆ (2.31.d)	
𐌺̆ (2.31.c)	𐌿̆ (2.31.f)
𐌺̆ (2.31.b)	𐌿̅ 𐌿̆ (2.31.g)
𐌿̆ (2.31.a)	

It should be noted, however, that we cannot be sure that the sound represented by 𐌿̆ was distinguished from the sound represented by 𐌺̆ by back articulation. If it is true, as some believe, that Proto-Germanic (and hence Gothic?) had umlauted vowels,<sup>38</sup> it seems quite likely that such a combination of symbols as *sokjan* may actually have represented something like [søkjan] in Gothic. Thus the primary feature of the opposition 𐌿̆ — 𐌺̆ may have been lip-rounding — lack of lip-rounding.

## 2.32 *Pronunciation of the consonant signs*

### 2.32.a 𐌺, 𐌿, 𐌿̅

As these signs represented voiceless stops in Greek, they were naturally articulated with greater force than the corresponding voiced sounds. This was an accidental feature of these three phonemes in Greek, but it is possible that it would have been considered the primary feature by a hearer whose language had only the opposition fortis — lenis. Since the signs 𐌺, 𐌿, were available to signal spirants, either voiceless or fortis, it is unlikely that these signs signal spirants. In discussing each sign, an attempt is made to give the range of possibilities of places of articulation which each sign could signal.

<sup>38</sup> See sect. 4.12. For a discussion of the theory of Proto-Germanic umlaut, see H. Penzl, "Umlaut and Secondary Umlaut in OHG", *Language*, XXVI (1950), p. 236f.

**Π** represented either a bi-labial or labio-dental voiceless or fortis stop.

**Τ** represented a voiceless or fortis stop, articulated anywhere from the inter-dental to the pre-palatal points of articulation.

**Κ** represented a voiceless or fortis stop, articulated anywhere from the palatal to the post-velar points of articulation.

### 2.32.b **β, δ, γ**

These signs are derived from **β, δ, γ** in the Greek alphabet, and they are used to render these in Biblical names and in loanwords. The phonemes represented by these signs were distinguished from those represented by **π, τ, κ** by the feature of voicing and, in most positions, by a spirant pronunciation. The phonemes represented by **β, δ** in Greek were distinguished from those represented by **φ, θ** by the feature of voice alone. We cannot be sure what feature and/or features distinguished **γ** from **χ** (cf. above, p. 27f.). The primary feature of **β, δ**, and probably **γ** in Greek is then seen to be the feature of voice. Since voiced sounds are commonly articulated with less force than voiceless sounds, the possibility exists that these signs could be used to render lenis sounds in a language which had the opposition fortis — lenis instead of the opposition voiceless — voiced. We must also reckon with the possibility that the signs **β, δ, γ** could have been used to render stops, and not spirants. In the following, we set down the range of possibilities of positions of articulation.

**β** represented a voiced or lenis stop or spirant, bi-labial or labio-dental.

**δ** represented a voiced or lenis stop or spirant, articulated anywhere from the inter-dental to the pre-palatal points of articulation.

**γ** represented a voiced or lenis stop or spirant, articulated anywhere from the palatal to post-velar points of articulation.

2.32.ba. — As **rr** is used to render Greek **γγ** in Biblical names and in loans, and the use of this digraph may have been borrowed from the Greek usage, **rr** doubtless signaled a nasal having the same place of articulation as the sound represented by **r**, plus the sound represented by **r**. Though **rk** is not found in the renderings of Biblical names, it seems likely that it also represents the sound signaled by **κ**, preceded by a homorganic nasal. It seems likely that **ra** represents a nasal plus the sound represented by **α**, but we cannot affirm this here (cf. 3.11.1).

### 2.32.c **h**

The use of **h** in Biblical names is so inconsistent that we cannot use the rendering of these names in our discussion of the pronunciation of this sign (see above, p. 26). Our only evidence for the pronunciation of **h** is therefore its derivation from the Latin alphabet. Since Latin *h* indicated merely aspiration, and possibly had many different positions of articulation, we may merely affirm that Gothic **h** probably signaled aspiration (see also 3.13.4.c).

2.32.d **ſ** and **z**

These derive from Latin *S* and Greek *ς*; they are used to render Greek *σ* and *ς* in Biblical names. Since the signs *θ* and *δ* were available to signal spirants, these Gothic signs doubtless signal sibilants. **ſ** represented a voiceless or fortis sibilant and **z** signaled the corresponding voiced sound.

2.32.e **λ**, **ƿ**, **μ**, **ν**

**μ** and **ν** are derived either from the Greek or the Latin alphabet; **λ** is derived from the Greek alphabet; **ƿ** is derived from the Latin alphabet. These signs render Greek *λ*, *ρ*, *μ*, *ν*, respectively, in Biblical names.

**μ** represented a bi-labial or labio-dental nasal.

**ν** represented a nasal articulated from the inter-dental to the pre-palatal points of articulation. That it did not signal a velar nasal is seen by the fact that **ṛ** was used for this purpose (2.32.ba).

**λ** represented a lateral and **ƿ** represented a vibrant. It is likely that both were apical sounds, but the possibility exists that they were not.

2.32.f **ƿ**

This sign is derived from the Latin alphabet. It is used to render Greek *φ* in Biblical names. Since Latin *f* represented a labio-dental spirant, whereas Greek *φ* represented a bi-labial spirant, it might be argued that Wulfila's use of **ƿ** indicates that Gothic had a labio-dental rather than a bi-labial spirant. It is usually assumed, however, that Wulfila decided not to use *φ* because of its resemblance to his *ψ*.<sup>39</sup> Since the Wulfilian form of *ψ* seems to have been something like *Ɀ* (1.14), this argument is invalid. It is quite possible that the use of **ƿ** is to be traced to post-Wulfilian scribes, for whom the resemblance between *φ* and *ψ* was too close. Since we are not sure of the derivation of the sign, the evidence of Biblical names alone should be used here. **ƿ** represented a bi-labial or labio-dental voiceless or fortis spirant.

It has been advanced that Wulfila's choice of a symbol other than Greek *θ* to render the sound represented by *ψ* shows that the sound represented by *theta* was foreign to Gothic.<sup>40</sup> Since it has been shown (1.14) above that Wulfila did use a form of Greek *theta*, this argument is no longer valid. *ψ* is derived from Greek *theta*, which it is used to render in Biblical names. *ψ* represented a voiceless or fortis spirant, articulated anywhere from the inter-dental to the pre-palatal points of articulation.

2.32.g. **Ϸ**

Since the origin of the sign **Ϸ** is uncertain (1.20), and since it is not used in the ren-

<sup>39</sup> Cf. Jellinek, p. 28; Bouuaert, p. 428; Hermann, p. 138.

<sup>40</sup> Cf. Hermann, p. 137.

derings of Biblical names, we have no way of determining its pronunciation at this point.

### 2.32.h $\alpha$

This was evidently a *k*-like sound with lip-rounding, as is seen by the fact that it derives from Latin *q* (1.12), and that it is used to render Greek  $\kappa\upsilon\upsilon$  (Latin *Qu*) in Biblical names. It may be that the fact that Wulfila left off the *u* indicates that the Gothic sound did not have as much lip-rounding as the Latin, or had no rounding, or it may be, as some have thought, that Wulfila wanted to indicate that  $\alpha$  represented a simple sound.<sup>41</sup> It may be that he was merely familiar with the grammatical tradition mentioned above (2.2 A).

### 2.32.i $\Psi$

This sign is derived from Greek  $\upsilon$ , which it also renders in Biblical names. The Gothic sign has a distribution which is quite different from that of Greek  $\upsilon$ , however. Whereas Greek  $\upsilon$  almost never occurs before vowels (it never occurs in that position in the Biblical names transcribed into Gothic), Gothic  $\Psi$  occurs predominantly in this position, and occurs between consonants only in final syllables. Although this distributional criterion is properly discussed in Chapter 4 and does not belong here, it may be used to indicate that Gothic  $\Psi$  represented a non-syllabic in most positions. It may thus have had the same articulatory features as one of the vowels (perhaps  $\mathfrak{u}$ ), having merely a different function in the language. Since it occurs in the same position as  $\mathfrak{u}$  (in final syllables, cf. Ph. 2,30 *daupu*, *gaidw*), however, it is likely that there was some feature which kept the two sounds apart.  $\Psi$  represented a lip-rounded non-syllabic in most positions.

### 2.32.j $\varsigma$

We cannot use the origin of the sign  $\varsigma$  to discuss its pronunciation, since its origin is uncertain (1.15). The fact that it renders Greek  $\tau$  and  $\epsilon\tau$  only in pre-vocalic position in Biblical names indicates that it must have been a non-syllabic. It must have had a relatively high position of articulation. The fact that  $\epsilon\tau$  before vowels is often rendered by  $\varsigma$ , but that  $\tau$  before vowels is only once rendered by  $\varsigma$  may be taken to indicate that  $\varsigma$  had a relatively higher position than  $\mathfrak{t}$ , perhaps with oral friction. The relative infrequency of  $\epsilon\tau$  before vowels (it occurs only 9 times in the Biblical names rendered in Gothic) deprives this assumption of any force, however.  $\varsigma$  represented a high (front) non-syllabic.

<sup>41</sup> Cf. H. Penzl, "Orthography and Phonemes in Wulfila's Gothic", *JEGPh*, XLIX (1950), p. 218f., and the authorities cited there.

## INTERNAL EVIDENCE I: ORTHOGRAPHICAL DEVIANTS

Besides the origin of the Gothic alphabet and the rendering of Greek sounds in Biblical names and in loanwords, internal Gothic evidence must also be considered.

In our discussion of internal evidence, we must distinguish between two main types: (1) evidence afforded by the deviations from the system of writing; (2) evidence afforded by the phonological system of Gothic itself, which can be broken down into two further types: (a) regular alternations; (b) the configuration of the system itself. As will be shown below, evidence of type (1) allows us to make conclusions concerning the pronunciation of the scribes only. Evidence of type (2a) allows us to make conclusions about a previous stage of the language alone, as will be shown in the next chapter. Evidence of type (2b) is the only evidence which allows us to make conclusions about Wulfila's speech. In the present chapter, we will discuss the evidence of type (1); evidence of type (2) is discussed in the next chapter.

*Note:* As there is no longer any need to insist on the forms of the alphabet as they are handed down in the mss., the usual system of transcription, as described in Jellinek, Streitberg, Braune-Helm, will be adhered to henceforth, unless there is good reason to depart from it.<sup>1</sup>

## 3.0 METHODOLOGICAL REMARKS

Counting erasures and superscript letters as errors, there are something over 1200 scribal errors in our Gothic manuscript material. Since the beginning of serious study of Gothic pronunciation, these have been seen as a means of determining certain points concerning the phonetic value of our Gothic signs.<sup>2</sup> If one seeks for a discussion of the methodology of handling these errors, one will seek in vain. Of previous discussions, let us cite Jellinek: "Die intern got. Erscheinungen können auf lautliche Zustände deuten, die recht verschiedenen Zeiten angehören",<sup>3</sup> and Streitberg:

Auch die orthographischen Schwankungen in den gotischen Handschriften können uns von Nutzen sein; ehe man sie zu Schlüssen verwertet, muss jedoch in jedem Falle erwogen

<sup>1</sup> Since the sequence *h-u* does not occur before vowels in Gothic, whereas **h** occurs only in this position, *hu* will be used henceforth in the place of the sign *h*, since this sign is so difficult to make on the typewriter.

<sup>2</sup> Cf., on *i/ei* for example, Gabelentz and Löbe, p. 55; cf. Weingaertner, # 20-25; Streitberg, # 33; Jellinek, p. 30f.; Braune-Helm, p. 4.

<sup>3</sup> Jellinek, p. 31.

werden, ob nicht etwa Einfluss romanischen Schreibgebrauchs die Ursache des Schwankens sei.<sup>4</sup>

These may be said to be typical examples of the type of discussion usually prefaced to consideration of the internal phenomena in the handbooks. Many handbooks (Braune-Helm, for example) give no methodological discussion, so that we do not know how they arrived at their conclusions.

### 3.01 *The scribes*

In discussing the scribal errors, we must not forget that Wulfila lived in the 4th century, whereas the bulk of our manuscript material comes from the 5th or 6th.<sup>5</sup> There is evidence of considerable activity in textual criticism and emendation among the post-Wulfilian Gothic clergy.<sup>6</sup> Furthermore, in spite of the assurance held in some quarters, we are by no means certain where the Gothic manuscripts were written

<sup>4</sup> Streitberg, # 33.

<sup>5</sup> Our manuscripts are not dated with any certainty. It is usually assumed, following Ludwig Traube, "Nomina Sacra", *Quellen und Untersuchungen zur lateinischen Philologie*, II (Munich, 1907), pp. 271 ff., that CA is early 6th, Car. 5th and the Ambrosian codices 6th. Traube dated the manuscripts on the basis of the abbreviations for sacred names found in them. Von Friesen and Grape, pp. 62-121, have attempted a more detailed dating of CA, and they too find that it is of the 6th century. Glaue and Helm believe the Giessen fragment to be of the 5th century (ca. 418), but others (cited Jellinek, p. 15) believe it to be of the 6th. Von Friesen and Grape believe that "Theodoriks erövnng av Italien" is a "terminus ante quem non" for the writing of CA, and Jellinek says (p. 15): "... ihre Entstehungszeit ist durch die Dauer des Ostgotenreichs festgelegt". "Wenn man etwa *car* ins 5. Jahrh. setzt und dabei an seiner italienischen Herkunft festhält, spricht man der Paläographie die Fähigkeit zu, das Alter einer Unzialhs. bis auf elf Jahre bestimmen zu können." Do they believe that there were no Gothic clergy in Italy before Theodoric? If so, why not seven years? The Goths must have been too busy from 489 to 493 (when Ravenna was taken) to have done much writing. What about the Arian disturbances of 485-487, and the Gothic court of Justina? What about Auxentius, Wulfila's foster son, who is thought to be the same as the Auxentius Mercurinus who opposed Ambrosius (cf. footnote directly below)? It should be further remarked that Theodoric was not very interested in church matters. Friedrichsen and Wilmart (cf. A. Wilmart, "Les évangiles gothiques", *Revue biblique*, XXXVI [1927], 44-61) believe it quite possible that the Goths with Athaulf used the Gothic Bible (Athaulf took Toulouse in 413 A.D.).

<sup>6</sup> This is seen by the *Praefatio* to the Codex Brixianus (f) (cf. M. Metlen, "A Natural Translation of the Praefatio attached to the Codex Brixianus", *JEGPh*, XXXVII [1938], 355-367, with notes), by the 106th letter of St. Jerome, addressed to two Gothic clergymen, Sunnia and Fretela who had asked him concerning parts of the Psalms (translated and explained by M. Metlen, "Letter of St. Jerome to the Gothic Clergymen Sunnia and Fripila", *JEGPh*, XXXVI [1937], 515-543), unless the two Goths are merely figments of St. Jerome's imagination, as D. de Bruyne ("La lettre de Jérôme a Sunnia et Fretela sur le Psautier", *ZfñW* [1929], 1-3) would have us believe; and by the fact that readings from the OL versions have crept into our text (cf. Friedrichsen, *The Gothic Version of the Gospels* [Oxford, 1926], p. 223, and *The Gothic Version of the Epistles* [Oxford, 1939], pp. 40-43, and the authorities cited there,) unless these were present in Wulfila's autograph (so H. Lietzmann, "Die Vorlage der got. Bibel", *ZfdA*, LII [1919], 249-278 and A. Jülicher, "Die griechische Vorlage der got. Bibel", *ZfdA*, LII [1910], 365-387, especially p. 379). The condition of our manuscripts points to a long tradition of writing. We may assume that Auxentius, if indeed it is he who aided Justina against Ambrosius (cf. K.K. Klein, "Ist der Wulfilabiograph Auxentius von Durostorum identisch mit dem Mailändischen Arianer bischof Auxentius Mercurinus?", *PBB*, LXXV [1952], 165-191) would have brought Wulfila's Bible into use among Justina's Goths. Cf. further A. Bezzenger, "Ueber die A-Reihe der got. Sprache" (Göttingen, 1874), p. 6f.



down. Following L. Traube, most Gothic scholars have accepted the assumption that the scribes who wrote our manuscripts were Ostrogothic.<sup>7</sup> Both Traube and von Friesen believed that they could show that the manuscripts were written down in Northern Italy, and Traube believed that they were “aller Wahrscheinlichkeit nach die Erzeugnisse derselben oberitalienischen Schreibschule”.<sup>8</sup> This viewpoint must be given up, since later scholarship has shown that there are actually two “hands”, one of which is closer to the Greek tradition than to the Latin.<sup>9</sup> Friedrich Kauffmann attacked the Ostrogothic hypothesis in 1899 on the grounds that there might well have been other Goths in Italy under Theodoric.<sup>10</sup> We know of Visigoths in Justinian’s court at Milan.<sup>11</sup> In a recent study, Michiel van den Hout has shown that the palimpsests were in all probability rescripted before they came to Bobbio.<sup>12</sup> A. R. Natale has shown that the upper script of the *Acta* of the First Council of Chalcedon (written over the *Skeireins*) exhibits French features, and that this manuscript was once in France.<sup>13</sup> Since no exhaustive study has been made of the suspension marks and the other marks of these codices, it must be left open here as to where they come from. Although it seems likely that they were not written down in Northern Italy, we will have to accept van den Hout’s decision that they were written down “in the northern half of Italy or the Danube countries”, at present.<sup>14</sup>

Even if we accept the thesis of Ostrogothic scribes, we are still in no position to affirm anything about their language. The attempt by Wrede to effect a description of their language using the transcriptions of names by classical authors has been attacked by Kauffmann on the grounds that Wrede had no criterion to judge whether the names he used were Ostrogothic or from some other Germanic tribe,<sup>15</sup> and by Kauffmann and Streitberg on the grounds that he did not take the Latin scribal tradition into account.<sup>16</sup> It is important then that we make no statements concerning language of the scribes which is not supported by the manuscript material, and that we exercise care in interpreting our manuscript material, always making sure that the assumption made about the language of the scribe is the only possible one.

<sup>7</sup> Cf. J. Janko, “Ueber germ. *ē* und sog. reduplizierenden Praeterita”, *IF*, XX (1906), 231 ff. The earlier authorities who believed in the Ostrogothic hypothesis are discussed in Wrede, *Ostgoten*, pp. 1-18.

<sup>8</sup> *Nomina Sacra*, 271 f.

<sup>9</sup> *Supra*, 1.01.

<sup>10</sup> In a review of the 9th ed. of Wrede-Stamm-Heyne, *ZfdPh*, XXXIII (1899), 92 f.

<sup>11</sup> Cf. K. K. Klein, *op. cit.*, 165, 172, 184 f.

<sup>12</sup> Michiel van den Hout, “Gothic Palimpsests of Bobbio”, *Scriptorium*, VI (1952), 91-93.

<sup>13</sup> A. R. Natale, *Arte e imitazione della scrittura insulare in Codice Bobbiesi* (Milan, 1950), p. 90. On the possibility of the Gothic Bible having been used in France, cf. above, footnote 5.

<sup>14</sup> Van den Hout, p. 92. Cf. in this respect also Capelle on the Veronese notes (p. 51): “De plus, et surtout, ils obligent à désormais nuancer une hypothèse formulée plusieurs fois à propos des fragments de Mai, à savoir que les documents ... viendraient d’un dossier transcrit par les moines de Bobbio. ... C’est ce codex même [i.e. the Veronese leaves] qui fut en usage dans une des communautés du Danube, vers l’époque à laquelle naguère dom Morin rapportait le mystérieux *opus imperfectum*.”

<sup>15</sup> Kauffmann, p. 92 ff.

<sup>16</sup> *Loc. cit.*, and Streitberg, # 16.3, *Anmerkung* 1.

### 3.02 *Are all errors errors of the scribes?*

Before we decide to charge all the errors found to the account of later scribes, it would be well to remember the words of Bernhardt, so often forgotten by later scholars:

Vielerlei umstände vereinigen sich jedoch auf diesem gebiete das kritische verfahren zu erschweren. ... wir dürfen doch schwerlich bei ihm (Wulfila) selbst volle consequenz in grammatischen und sprachlichen dingen voraussetzen, folglich darf sie ihm auch nicht aufgezwungen werden; aber es ist oft nicht leicht die grenze zwischen dem schreibfehler und der in der natur der sprache begründeten abweichung zu ziehen.<sup>17</sup>

We naturally expect Wulfila to have been consistent in his practice, but any person who has done any amount of writing in a system of his own devising, even with the express purpose of conveying information to others (as in field work, for example), knows that we are not always consistent, but change from day to day, and, forgetting the practice followed yesterday, fall back into the old practice today. As Wilhelm Braun put it, in discussing orthographical practices in B: “Wer schreibt an allen Tagen gleich?”<sup>18</sup> I know of no criterion whereby we may decide whether a given error is Wulfilian or scribal. Probably the most sensible approach, in the face of the two hundred years of scribal activity lying between Wulfila and our manuscripts, is to assume that any Wulfilian “errors” would have been leveled out, and that all errors are scribal. This is, however, merely an assumption.

### 3.03 *What do scribal errors tell us about Wulfila’s pronunciation?*

It is usually affirmed in the handbooks that scribal errors afford us an indication as to Wulfila’s pronunciation.<sup>19</sup> Even those people who believe most firmly in the Ostrogothic hypothesis believe firmly that scribal errors afford us a direct witness as to Wulfila’s pronunciation. Thus J. Janko, in an article on Germanic *ē*, affirms that the writing of *ei* for *e* is Ostrogothic, but continues, “... für das Wulfilanische folgt daraus einzig und allein die sehr geschlossene Aussprache seines *ē* überhaupt”.<sup>20</sup> It is difficult to see how the pronunciation of one dialect, at a remove of two centuries, can tell us much about the absolute pronunciation of another. It is not an uncommon experience to find two phonemes held apart in one dialect, whereas they have fallen together in another. Only if we assume that the scribes were members of the same dialect group as Wulfila can we affirm that the falling together of two “sounds” allows us to make conclusions concerning Wulfila’s speech. But the fact that we find

<sup>17</sup> Bernhardt, *Wulfila*, p. 1 f.

<sup>18</sup> Streitberg, *Bibel*, 482.




<sup>19</sup> Cf. Streitberg, p. 55: “Für enge, dem *i* nahestehende Qualität [of *e*] spricht ... die Vertauschung von *e* mit *ei* (und *i*) in den got. Hss.” Jelinek, p. 45: “Dass *i* und *ei* einander nahestanden, lehren gelegentliche, wenn auch seltne Vertauschungen ...” Cf. further Hirt, “Grammatische Miscellen”, *PBB*, XVIII (1984), 280, footnote 1.

<sup>20</sup> Janko, “Germanisches *ē*”, 231 f.

that two “sounds” have fallen together in a later stage of a language does not mean that they were, at any given point in their earlier history, phonetically quite similar.

### 3.04 *Do all scribal errors afford us evidence of pronunciation?*

There are several ways in which scribal errors creep into our manuscripts. Firstly, there are mechanical errors. These are of several types. The scribe may repeat signs or leave them out under the influence of the surrounding signs. In such cases, we usually say that the scribe’s eye wandered. We often find that whole lines will be skipped because of this phenomenon. This is the most common type of mechanical error. The scribe may be deceived by the fact that one sign resembles another, especially if the sense of the resulting word is close to that of the original. It may happen that the scribe, in his wish to be consistent, may be influenced by a remembrance of the “Schriftbild” *per se*; that is, may reproduce an aberrant form which he has seen before. In such a manner an isolated instance of a mechanical error may be perpetuated in the text until it is noticed by a second scribe. Another type of “error” occurs when a scribe who finds the sense of a passage destroyed for him by a certain form changes this form in such a manner as to make the passage more meaningful to him.<sup>21</sup> Such errors are by far not unusual.

Another mechanical error which has perhaps not received enough attention in the past occurs when the scribe is not familiar enough with the orthographic practices of the scribe whose manuscript he is copying. Thus, it is often affirmed that final *-m* seems to have had a tendency to become *-n* in later Gothic, since we often find *-n* written for *-m* in our manuscripts.<sup>22</sup> It should be noted, however, that some of our manuscripts use a suspension sign for both *m* and *n*, whereas others use the suspension sign only for *n*. If a copyist happened not to be familiar with the conventions, he might take the sign  to be simply a variant of the sign , and thus to indicate *n*. The same may be said of such errors as *þairgaleikoda* (K 4,6 A) for *þairhgaleikonda*. A number of such errors could be taken to mean that *on* and *o* had fallen together in position before consonants, if we did not know that it is the practice in B, and perhaps in the archetype of A and B<sup>23</sup>, to use a suspension mark for *n* in such cases (cf. *ingalei-kōda* k 3, 18 B). It may well be the same for the well-known simplification of *nd* to *n* in B.<sup>24</sup> A ligature  is used for the combination *nd* in some of our Gothic manuscripts. A person unfamiliar with this practice could well mistake this ligature for an accidental variant of *n*, thus rendering it as *n* in his copy. The same may be said of

<sup>21</sup> Such errors are not infrequent in our Bible mss., cf. Westcott and Hort, pp. 541-545.

<sup>22</sup> Cf. Bethge, # 33.

<sup>23</sup> A and B are usually supposed to have the same archetype, since they have a number of errors in common (cf. G 5,21 *faurqiþa* for *faura* C 4,6 *gasuqoþ* for *gasupob*; C 4,10 *du* for *bi*; E 1,14 *arþjos* for *arþjis*). It should be noted that B uses four times as many suspension marks (always for *n*) as does A. Suspension marks are used in a strange manner in B, so that we find such examples as *gaiþjandans* for *gairn-* (T 6,10), *qineia* for *qineina* (t 3,6), *mañ* for *mann* (E 2,15), *wrikāai* for *wrikanai* (k 4,9).

<sup>24</sup> Bethge, # 130; Bernhardt, *Vulfila*, p. lvii.

the loss of final *-s* after *n* (cf. C 3, 12 B *bleiþein* for *bleiþeins*, Sk. 8,17 *baitrein* for *baitreins*). The ligature *is* could have been mistaken for *n*.<sup>25</sup> It is well to note here that the manuscript which has the ligatures does not have the errors.

Errors may have also crept in because of the condition of the material which bore the writing. It is not unusual to find scholars misreading papyrus where the ink has run.<sup>26</sup> In such cases the scribes also may have misread the archetype. Some scribes follow the custom of writing around holes instead of ending a word before the hole and beginning the next after it.<sup>27</sup> In such cases, later scribes may try to fill out the space left by the hole and thus make errors. If the manuscript is a palimpsest, the other writing may cause the scribe to err. Scholars have been fooled by palimpsests, and we cannot expect the scribes to be more discerning.<sup>28</sup> If the material written on is vellum, we can expect the scribes to have been fooled by folds in the manuscript. It was a common practice in those days to push the vellum over a hole in the pressing of it.<sup>29</sup> If such a place became roughed up in the process of pumicing the vellum before using it, it could be written on and then pressed down again. Such a place is seen in Ph. 2,25, B, where Braun's inspection found a fold in the manuscript to have caused Uppström to read a *b* as an *f*.<sup>30</sup> All these points must be taken into consideration before we assume that an orthographical deviant indicates an actual pronunciation.

There are errors which give evidence of pronunciation, however. The scribe, in copying from a document which contains graphic signs representing sounds different from those of his own dialect will, if the two dialects are sufficiently close, unwittingly introduces graphic signs for the sounds of his own dialect. This can, *a priori*, occur in two basic ways: (1) the scribe may be copying a manuscript and attempting to reproduce faithfully the signs of this manuscript; or, (2) he may be attempting to change certain parts of the manuscript to make it conform better to his own dialect. It is uncertain if the latter practice ever occurs in texts of a religious nature, but it must be considered.

### 3.05 What are scribal errors?

Because we have so little manuscript material, we are not in a position always to decide which deviant is the error and which is the correct, or Wulfilian, form. In previous treatments, etymology has been allowed to influence the decision.<sup>31</sup> This

<sup>25</sup> On the ligatures in A, cf. von Friesen and Grape, p. 99.

<sup>26</sup> Cf. Massmann, p. 6f.

<sup>27</sup> An example of just such a practice is seen in B, Ph. 2,25, where *broþar* is written *b—roþar* because of a hole in the parchment.

<sup>28</sup> An example of a scholar being fooled by the upper writing on a palimpsest may be seen in Uppström's reading of *sijum* for *sium* (k 2,17). What he thought to be the curve of a *j* was actually a part of the Latin text (Streitberg, *Bibel*, p. 289).

<sup>29</sup> On the manufacture and preparation of vellum, cf. Wattenbach, *Das Schriftwesen im Mittelalter*, 2nd ed. (Berlin 1879), p. 102ff.

<sup>30</sup> Streitberg, *Bibel*, 371: "... das *b* steht in den Falten des um ein Loch verzogenen Pergaments ...".

<sup>31</sup> Cf. for example Streitberg, p. 48: "Hierher auch *wriþus* L 8,33 wegen ac. *wræd* 'zwölf Stück Schweine'". "Vielleicht *afdobn* L 4,35 für \**afdubn*, vgl. aisl. *dofna* ..."

practice should be abandoned. A point which has not previously received notice is that we have a number of emendations and erasures made in each case by the scribe's own hand.<sup>32</sup> Such emendations, of which there are ca. 150 in our material, should be given the status of high authority in determining what things were considered errors by the scribes themselves. We may say that, in general, a correction implies that the scribe considered that which he was correcting to be wrong, and that which he was adding to be right. This may be said to be our best type of evidence, and it should head our discussion of each type of error. Changes made by other scribes, contemporaneous with or nearly contemporaneous with the first, are to be accorded almost equal authority. We must always suspect the authority of later scribes. Scribe III of Aleph and Uppström's "nefarius corruptor" are outstanding examples of the blundering of later scribes.<sup>33</sup> We should further not speak of our manuscripts as if they were written all by one scribe. It is necessary to distinguish between scribes wherever possible. It is often affirmed that Ambr. A differs from Ambr. B in that it has such forms as *nuk-kant* for *nuh-kant* (K 7,16), *jas sunjos* for *jah sunjos* (K 5,8), etc.<sup>34</sup> It should be noted, however, that, except for *nis-sijai* (R 7,13; R 11,1; R 11,11, found also in CA L 20,16) and two other examples, all the errors of this type in A are found in Car. and in Hand III.<sup>35</sup> Another example: It is affirmed by Streitberg, following Braun, that C 1,16 B and th 3,17 B have abbreviation marks for *-m* (*himinam* and *epistaulem*), though Uppström found none there.<sup>36</sup> If these two cases proved to be true, they would be the only cases in which B has used a suspension mark for final *-m*, out of ca. 100 examples. This seems to make Braun's reading suspect, especially when we remember that final *-m* and *-n* are occasionally confused in our mss. As the grand old masters of NT textual criticism have put it: "Its principle may be expressed in a single proposition, *Knowledge of documents should precede final judgement upon readings*."<sup>37</sup>

### 3.1 THE ERRORS AND THEIR INTERPRETATION

#### 3.10 *Types of errors*

We must distinguish three main types of errors. Mechanical errors, which tell us nothing about the pronunciation; errors which may be mechanical, and which tell us little about pronunciation; errors which afford evidence as to pronunciation. At the

<sup>32</sup> On the erasures and emendations in CA and the manner in which they are done, cf. von Friesen and Grape, pp. 56-59. The erasures in the Ambrosian codices are all reported in Streitberg, *Bibel*, according to the readings of Wilhelm Braun. They are found at the bottom of each page.

<sup>33</sup> On Uppström's "nefarius corruptor" cf. his edition of CA (notes on the following places: M 8,24; M 9,24; J 11,12; J 11,13; J 18,40; L 8,33; L 9,32; Mc. 4,7; J 10,23). This corruptor has been shown by von Friesen and Grape (p. 153f.) to be of the 17th century.

<sup>34</sup> Streitberg, # 26; Bernhard, *Vulfilā*, liii.

<sup>35</sup> On the hands of A, cf. Streitberg, *Bibel*, 481f.

<sup>36</sup> See Streitberg's notes to these citations, *Bibel*.

<sup>37</sup> Westcott and Hort, p. 543, italics theirs.

end of the section, I add a group of errors which are of unknown value, being errors introduced by various causes, which can no longer be discovered with any degree of certainty.

### 3.11 *Mechanical errors*<sup>38</sup>

Mechanical errors are of several types, and they are called by various names. The nomenclature used here is in general use and is self-explanatory.

#### 3.11.1 *Uncertainty in the writing of the velar nasal*<sup>39</sup>

Since writing *g* for the *n*-phoneme before *k* or *g* is signaling a sub-phonemic variation, it is understandable that the scribes wavered in the writing of it.<sup>40</sup> We find that the *g* is often left out entirely: C 2,16 B *þukeiþ* with a *g* inserted before the *k* above the line; Sk. I, 18 *gaagwein* for *gaaggwein* (L 8,3, L 16,1, etc.) *ng* and *nk* for *gg* and *gk*: L 15,22 *bringiþ*, L 15,23 *bringandans*; L 17,19 *þank* for *þagk*, L 19,31 *inqis*. Occasionally the *g* is doubled: M 9,15 *atgagggand* (not noted in the notes to Streitberg's edition), T 6,9 A *saggqjand*, and occasionally the *k* is then left off: L 5,39 *driggandane*, E 5,18 *anadriggaiþ*. We have no other evidence pointing to a voicing of medial *k*, so we merely assume that this is graphic. The usual view of these writings is that they are merely slips of the scribe's pen. It is often affirmed that the writing of *n* for *g* here points to Latin influence.<sup>41</sup> This is not necessarily true. It is quite likely that the scribes wrote *n* because the velar nasal was merely a sub-phonemic variant of it.

#### 3.11.2 *Reversal of letters and syllables; scribal spoonerisms*

We find in ca. 15 places in our material that letters and syllables have been reversed: G 4,11 *taur. arbaidedidjau*, Mc 14,13 *gaggast* for *-ats*, L 14,29 *mahtide* for *-edi*, K 9,24 A *spraud* for *spaurd*, k 7,1 *bilauseino* B for *bisauleino* A, etc., G 2,1 we find that A (*taur.*) has the number .di. (a meaningless combination) for .id. ('fourteen', found correctly in B). What is perhaps a spoonerism is found L 10,5 *in þane gardei* for *in þanei garde* (cf. Streitberg's note in *Bibel*).

#### 3.11.3 *Dittography*

Ca. twenty examples of dittography are found in our material: G 2,1 *fidwortaihune*

<sup>38</sup> Mechanical errors are discussed in Bernhardt, *Vulfila*, 1-lxvii, and in Gaebeler, 5-9. Both these discussions are excellent. They are not arranged in the order found here, nor are they in any way complete in their material. The material found in my 3.11.6 and 3.11.7 is not discussed by them. The writing of the velar nasal is not discussed by Gaebeler.

<sup>39</sup> Cf., in addition to Bernhardt, cited above, Streitberg, # 28. Neither of these gives the complete material.

<sup>40</sup> I am perhaps getting ahead of myself here, but it cannot influence my interpretation of Gothic pronunciation. On the phonemes of Gothic, cf. below, section 4.3.

<sup>41</sup> W. Schulze, "Orthographicum", *Glotta*, XVI (1927), p. 304.

*jera/jera* (*e jera* later erased, the citation is from A, *taur.*), *afafgaggandam* (pl. 33 line 11 of von Friesen and Grape's phototypical edition, not mentioned by Streitberg) with *af-* later erased, M 5,31 *huahuazuh*; J 12,47 *mananased*, etc., etc. An example of dittography of a single letter is seen M 6,6 *fulhlsnja*.

#### 3.11.4 *Loss*

There are ca. 50 examples of letters and syllables being left out in our material, of which ca. one-third have been corrected: C 1, 29 B *usdau<sup>d</sup>jans*; Sk. II,17 *piudan<sup>g</sup>ardja*; Sk. II,10 *man<sup>na</sup>*; Th 2,15 *prauf<sup>e</sup>tum*; Ph 4,14 *briggan<sup>da</sup>ns* (B). The error which consists in leaving off a final-*-dan-* is a very common one, but it probably does not reflect a change in pronunciation (cf. M 26,73; Mc 4,1; Sk. VI,3; t 3,8; G 3,3; but also G 5,7, where we find the final *ans* on *ufhausjandans* erased). Other interesting errors of this type are: Mc 2,16 *fraurhtaim* for *frawaurhtaim*; *gaiwa* for *gaidwa* C 1,24 B (the correct form is found in A); the subscript to C has *Kaussaim* for *Kaulaussaium* (cf. Streitberg's remarks in *Bibel*); G 1,24 *melidedun* for *mikilidedun*.

#### 3.11.5 *Additions*

Ca. 20 times we find letters and whole syllables added without apparent reason: R 12,2 A *framajjis* changed to *frajjis* by erasure, Mc 6,19 *naiswor* changed to *naiw*; k 6,7 B *taihsnwona* (A right). The addition of *f* in *swumfsl* (later erased) J 9,7 may indicate an actual pronunciation, cf. below 3.12.3.

#### 3.11.6 *Letters lost because of other letters near-by*

In ca. 25 places we find that letters were lost because of what preceded or followed them: Sk. IV, 20 *an airpai*, T 2,2 B *triggwaimannam* for *-aim m-*, Th 5,7 B *nahtslepan* for *-ts sl-*; often *us-* for *uss-* (L 8,55; L 10,25; L 14,14; Mc 3, 13; Mc 10,34); this may actually mirror a pronunciation feature. Streitberg (≠ 33b) believes that L 16,6 *fim-tigjuns* is a reflex of the loss of *f* in the cluster *mft*; this is doubtful.<sup>42</sup>

#### 3.11.7 *Letters added because of other letters near-by*

Ca. 15 cases: k 7,11 *saihua auk* for *saihu auk*; M 7,16 *lisanda af* for *-d af*; J 13,35 *ufkunnanda allai* for *-nd al-*. A number of examples are seen in Sk. IV, 21-24.

#### 3.11.8 *Letters miswritten for others*

Ca. 20 examples: Neh 6,16 *ausona* changed to *augona* (perhaps a case of a word mis-

<sup>42</sup> In this he is following W. Schulze, "Gotica", *ZvS*, XL (1909), 92. Schulze's argument is etymological and cannot be allowed here.

written for another); M 7,13 *wigss* changed to *wigis*; Ph 3,21 B *wusþaus* for *wulþaus*; Th 3,3 B *ratidai* for *satidai*. The example *þleihsam* (k 12,10 B) for *þreihslam* (A) may actually indicate dissimilation.

### 3.12 Errors which may indicate pronunciation

#### 3.12.1 *ns* for *nds* and *nds* for *ns*<sup>43</sup>

This error occurs 14 times in our material: cf. E 2,16 B *afslahans* for *-ands* (A); T 3,4 B *fauragaggans* for *-ands* (A); T 1,3 *galeiþans* (B) for *-ands* (A) — L 10,2 *waurstw-jands* for *-ans*. It is usually thought that this alternation indicates that *ns* and *nds* had fallen together.<sup>44</sup> This may be true, but it is by no means certain. We should notice that 12 of these errors are found in B, whereas 2 occur in A, 1 in CA. We find in A a ligature **𐌺** used for *nd*, as mentioned before.<sup>45</sup> If a scribe were not familiar with this convention, he might read it as simple *n*.

#### 3.12.2 *Simplification of geminate clusters and geminations*

3.12.2.a *Simplification of geminate clusters* — This phenomenon occurs 16 times, mostly (11 times) for *l* or *m*: T 1,16 *duþbe* B; R 14,3 *frakuni* — since previous studies have not given the material, I cite the full material here: L 2,21.22; L 2,43; L 8,43; L 9,51; L 14,23; Mc 5,37; K 9,26 A, E 3,19 A; T 5,19 A; Sk. V,12; Sk. III,22; T 5,22 B.

3.12.2.b *Geminations* — These occur 9 times, mostly for *l* or *m*. Ph 4,15 B *ainnohun*; k 6,7 A *hleidumonna* (B right). The complete material: Sk. VI, 19; t 3,16 *alll* for *all*; G 5,15 B; K 9,21.22; L 2,46 *allh* for *alh*; J 19,2; k 11,14.

The number of cases is not large enough to permit us to make any conclusions. The cases observed may indicate that long consonants were simplified in late Gothic.

#### 3.12.3 *Simplification of clusters and additions of sounds in clusters*

These two phenomena perhaps point to a simplification of clusters in the speech of the scribes.

3.12.3.a *Simplifications of clusters* — *-st-*: t 3,10 marginal note *galaisides*; T 6,4 *haifteis*; Ph 1,15 B *haiftais*; J 6,28 *waurswa*; of consonant plus *j*: Th 5,14 *broþʳjus*; G 6,12 *wrekʳa* (A); Mc 16,2 *attiddedun*; Th 4,13 B *broþrus*; complete material: Sk. VI,2; Sk. IV,13; t 1,10 B (a mistake of the archetype, since A has *aiwaggeþjon*); Th

<sup>43</sup> Cf. Bernhardt, *Vulfila*, lvii; Bethge, # 130. To Bethge's examples should be added: E 2,16 B (A right); T 3,4 B (A right); k 9,4 *unmannjands* for *-ans* (A). Bernhardt does not attempt to give the full material.

<sup>44</sup> Cf. Bethge, # 130.

<sup>45</sup> Cf. above 3.04. For examples of the ligatures, see von Friesen and Grape, p. 99.



5,5; G 4,15 A (*taur.*); E 6,10; L 1,29; G 5,9 B; L 6,36; J 18,9; M 6,4; L 20,37.

3.12.3.b — *Additions in clusters* occur only three times: J 9,7 *swumfsl*, with *f* later erased; k 6,7 *taihsnwona* (B, A right); M 6,6 *fulhlsnja*. This last example, taken with M 6,4 *fulhsja* and J 6,28 *waurswa* seems to indicate that five consonant clusters were simplified in colloquial speech. Such clusters occur only in three words: *waurstwja* (15 occurrences), *fulhsnja* (4), *huilfirjom* (once).<sup>46</sup>

### 3.12.4 *Loss and addition of nasals*

These fall into three classes: internal loss or addition; confusion of final *m* and *n*; loss and addition of final *n*.

3.12.4.a *Internal loss and addition* — Loss: K 15,6 *fi<sup>m</sup>f*; L 17,22 *sununs* for *sunaus*, Bernhardt thinks that perhaps *sunus* was miswritten for *sunaus* (as often, cf. above, 3.12.3) and then misread as *sununs* (*sunūs*); k 11,28; T 5,7 B; L 14,31; Sk. V,3; -*s* for -*ns* R 11,24; T 4,14; th 1,8 A; E 2,4. As mentioned above, this last phenomenon may be caused by the ligature *ns* being misread as simply -*s*.<sup>47</sup> The other examples may have come about by misreading of suspension marks, since each ms. has its own peculiarities. C 1,16 and th 3,17 (both in B) cannot have suspension marks for -*m*.<sup>48</sup> Addition: E 4,29 B *unbilaize* (perhaps an attempt was made to erase the *n*, Streitberg *Bibel*); R 11,18 *anstans* for *astans* (no note in Streitberg, *Bibel*); K 4,6. All these doubtless came about because of misreadings for suspensions.

3.12.4.b *Confusion of final m and n*.<sup>49</sup> — -*n* for -*m*: Mc. 5,3 *aurahjon* changed to -*m*; E 3,20; E 2,2 B; Mc. 9,28; L 7,24; Mc. 1,38; Mc. 4,11; Mc. 2,3. -*m* for -*n*: L 9,36, in the margin; k 7,3; G 6,12; G 2,5; M 25,44 *sehuun* for -*um*, but Streitberg, *Bibel*, says that the abbreviation (suspension) is for -*m* and not -*n* (so Bernhardt, p. 36). As this last example shows, this error is probably merely the result of misreadings of suspension marks; if two scholars could differ, why not two scribes?

3.12.4.c *Loss and addition of final -n* — Loss: C 1,16 *waldufnja<sup>n</sup>*; t 2,22; k 12,8. Addition: C 2,20 *huan* with *n* erased; J 7,51; K 7,16; J 6,10, k 10,2. In my opinion, these are to be judged as confusions of suspension marks and slips of the pen.

### 3.12.5 *t for þ and þ for t*

*t* is written 6 times for *þ*.<sup>50</sup> Cf. L 5,11 *afleiþandans* for *aflet-*; G 2,11 *gaparhiþs* for

<sup>46</sup> See the sound statistics given by Jellinek, p. 55.

<sup>47</sup> Cf. above, 3.04.

<sup>48</sup> Cf. above, 3.05.

<sup>49</sup> Bethge, # 130.

<sup>50</sup> Cf. Braune-Helm, p. 41; Bethge, # 130. T 3,16 A *unsahþaba* should probably be added to their list (Cf. Streitberg, *Bibel*).

*gat-*. This is thought by Bethge to indicate that Late Gothic *t* was aspirated.<sup>51</sup> Two of the five and perhaps a third can be explained as simple misreadings, however, and it is doubtful if this error may be taken as evidence of pronunciation. If we could assume aspiration in Late Gothic, this might be made to mean something for Wulfila's Gothic. In k 12,7 we find *hnuþo* in A and *hmuto* in B. As this is a *hapax legomenon*, we cannot tell which is the error.

### 3.12.6 *b, d, z for final f, þ, s*<sup>52</sup>

We find *-b* and *-bs* written in our material 20 and 5 times, respectively, where we should expect *-f* and *-fs*: th 3,12 (AB) *hlaib*; Sk. II,15 *gadob*; L 4,3 *hlaibs*. We find *-d* and *-ds* 176 and 60 times, respectively, where we should expect *-þ* and *-þs*. We find *-z* 9 times where we should expect *-s*. These errors are often supposed to indicate a pronunciation feature. Wrede's assumption of two different types of *þ* and *f* has not found any acceptance.<sup>53</sup> Sievers and Streitberg thought that these errors were caused by *f, þ, s* becoming voiced before words beginning in a voiced consonant or vowel.<sup>54</sup> The fact that *b, d, z* appear occasionally before voiceless consonants (T 4,11 B *anabiud þata*; L 6,32 *frijod þans*; L 6,33 *taujaid þaim*) shows that this alternation is not conditioned by the voiced beginning of the next word.<sup>55</sup> As Hench has pointed out, it is natural that this error should occur more often before voiced sounds, since Gothic words begin more often in voiced sounds than in voiceless.<sup>56</sup> Streitberg's rejoinder (*IF*, XXIV, 175-181) that it is the ratio which should be the most important consideration cannot be seriously considered. The theory that this shows Ostrogothic voicing of spirants in final unaccented syllables cannot be used to explain such examples as *gadob, sad, god*, etc.<sup>57</sup> The most sensible approach, and the one which has most widely found acceptance, is to consider this as a sort of analogical spelling, "morpho-

<sup>51</sup> Bethge, # 130.

<sup>52</sup> Collected by Streitberg, "z, b, d im Auslaut", *IF*, XVII (1906), 396ff. L 9,48 *grob*; J 14,2.3 *stad*; J 14,31 *anabaud*; K 15,34 A *frawaurkjaid*, all of which support his theory, should be added (cf. Jellinek, p. 57).

<sup>53</sup> Cf. F. Wrede, *Stamm-Heyne's Ulfilas, oder die uns erhaltenen Denkmäler der gotischen Sprache*, 13th and 14th ed. (Paderborn, 1920), # 58. On this assumption, cf. Hench, "The Voiced Spirants in Gothic", *JGPh*, I (1897), 56.

<sup>54</sup> Streitberg, "z, b, d im Auslaut", "Zum Sievers'schen Gesetz", *IF*, XXIV (1909), 175-181. W. Braun, "Ein satzphonetisches Gesetz des Gotischen mit vorwiegender Rücksicht auf die Codices Ambrosiani", *GRM*, V (1913), 367-391, defends this hypothesis using his new readings of the Ambrosian codices and his theory of stichometric writing.

<sup>55</sup> Cf. H. Penzl, "Orthography and Phonemes in Wulfila's Gothic", *JEGPh*, XLIX (1950), 224; Hench, p. 51.

<sup>56</sup> Hench, p. 51. Hench's idea that the ratio of voiced to voiceless beginnings was 6:4 is wrong. According to the figures cited by M. Joos, "Statistical patterns in Gothic Phonology", *Language*, XVIII (1942), 34, voiced beginnings are found in 37,945 items, voiceless in 17,774 items. The ratio is ca. 2:1.

<sup>57</sup> This theory was first put forth by A. Kock, "Ein konsonantisches Auslautsgesetz des Gotischen aus dem Accent erklärt", *ZfdA*, XXV (1881), pp. 226-232. Cf. also his article "Zur Frage nach dem Wechsel zwischen stimmlosen und stimmhaften Fricativae", *ZvS*, XXXVI (1900), pp. 571-579. See the authorities cited by Penzl, p. 223, footnote 23.

phonemic spelling”.<sup>58</sup> If we could consider the language of the scribes as a continuation of Wulfila’s dialect, the fact that *þ* and *d*, *f* and *b* still seem to form a correlative pair would be significant.<sup>59</sup>

### 3.12.7 *ij/j*<sup>60</sup>

We find *i* and *ij* both used before vowels in the words usually cited as *sium*, *siuþ*, *siau*, *siais*, *siai*, *saian*, *faian*, *armaio*, *frijon*, *frijaþwa*, *fijan*. Since the same phenomenon is found in the transcription of Biblical names, it is thought by Streitberg to signal an “Übergangslaut”.<sup>61</sup> Pisani feels that the writing *saijīþ* shows that the digraph *ai* was a diphthong with a high front component, since *j* could be left out in such forms as *saiiþ* (*saiiþ* “valeva quanto” *saijīþ*). This proves, according to him, that *ai* had an *i* sound, and that the “Übergangslaut”, since it was determined by *i* plus *i*, did not need to be signaled in the writing system.<sup>62</sup> If we confront OHG *sāen* with Late OHG *sājan*, however, we see that it is not necessary to have an *i* sound for a *j* to develop as an “Übergangslaut”.<sup>63</sup>

### 3.12.8 *Minor errors in writing vowels*<sup>64</sup>

3.12.8.a *i* and *a*. — We find several times in our mss. *a* written where we should expect *i* and vice versa: *a* for *i*: Mc 10,29 *attin* for *attan*; L 15,9 *drakmin* for *drakman*; L 1,5 *gudji* for *-ja*; R 14,3 A *matjandin* for *-an*; G 2,18 *missataujandin* for *-dan*. G 2,6 *wulþ<sup>a</sup>is* (B, A right) *i* for *a*: M 27,64 *þridjan* for *-jin*; J 11,24 *spedistan* for *-in*; k 11,2 *usgaban* for *usgiban* (B).

3.12.8.b *a/ai*. — *a* for *ai*: Mc 6,11 *janai* for *jainai*, G 2,9 B *þa<sup>t</sup>ei*. *ai* for *a*: Ph 2,29 G *haibaiþ*; Ph 2,5 *fraiþjaidau*; K 7,11 *unliugadai* for *-da*; L 18,11 *inwindai* for *-da*; k 5,15 B (A right) *libainai* for *-na*.

3.12.8.c *i/o*: — Signature of K (A) *silbons* for *-bins*, E 1,14 (AB) *arbjos* for *arbjis*; k 9,15 *unusspillidons* for *-spillodons*; L 1,23 *dagis* for *dagos*.

3.12.8.d *ei/ai*: — Sk. I,10 *þatine* changed to *þataine*; L 14,32 *eipþau*; k 8, 18 A *þizai* for *þizei* (B).

<sup>58</sup> Hench, Bethge, p. 200, 400; Moulton, “The Stops and Spirants of Early Germanic”, *Language*, XXX (1954), 4, footnote 10; Penzl, p. 223.

<sup>59</sup> Cf. the remarks of Jellinek, p. 57f.

<sup>60</sup> The material is gathered in an article by Hermann Jacobsohn, “Zwei Probleme der got. Lautgeschichte”, *ZvS*, XLVII (1916), p. 83ff. I noted no errors.

<sup>61</sup> Streitberg, # 30.

<sup>62</sup> Vittore Pisani, “La pronunzia di *ai*, *au* in gotica, a proposito di una recente pubblicazione”, *Paideia*, IV (1949), p. 119f.

<sup>63</sup> Cf. O. Mausser, *Mhd. Grammatik* (Munich, 1933), p. 87.

<sup>64</sup> Cf. Gaebeler, p. 6f. The counts are unreliable.

3.12.8.e *ai/au*: T 6, 15 B *fraiimondane* for *frauji-*; k 10,2 *gatraiau* for *gatrauau*; k 12,18 B *laustim* for *laistim* (A).

3.12.8.f *i/ai*: t 3,11 A *uṣṣulida* (B right); Th 4,14 *tiuhaiṣ* for *-iṣ*. K 7, 16 A *Aganasj<sup>a</sup>is*.

3.12.8.g *a* for *au*: *gaaiwiskonda* for *-dau*, k 9,4 A (B right); L 4,17 *prafetus*; k 3,10 *tauja* for *-jau*; the three other examples, given by Gaebeler, p. 6f. are all spurious, cf. Streitberg, *Bibel*.

3.12.8.h *a* for *u*: J 13,12 *witadu* for *-udu*; R 13,13 A *dragkaneim* changed to *drugk-*.

3.12.8.i *a/o*: M 7,23 *unsibjana* for *-ona* (Leo Meyer, p. 346, maintains that this is no error — Bernhardt, Streitberg, Wrede say it is); *ainoho* for *ainaho* (cf. Streitberg, *Bibel*, p. 125).

3.12.8.j *ei* for *a*: Ph 1,29 B *ḥatei* for *ḥata*.

3.12.8.k *i* for *au*: M 10,41 *praufetis* for *-taus*.

Taken singly, perhaps these examples afford us little in the way of material from which to decipher pronunciation. Taken as a whole, they perhaps indicate a weakening of endings in the language of the scribes.

### 3.12.9 *Minor errors in writing consonants*

3.12.9.a *Loss and addition of final ḥ*. Th 2,13 B *waurkei* for *-eiḥ*; C 3,15 *swignjaiḥ* for *-jai*.

3.12.9.b *t* for *d*: C 1,18 A *frumatein* for *-adein* (B).

## 3.13 *Errors indicating pronunciation*

The following errors are thought to indicate that certain sounds have fallen together in the language of the scribes.

### 3.13.1 *e/ei/i confused*

3.13.1.a *ei* for *e*.<sup>65</sup> — This error occurs 67 times in our material: L 5,31 *leikeis*; M 9,6 *afleitan*; L 1,5 *qeins*, etc., etc. It should be noted that it occurs in stem syllables only in 14 words. Bethge has noted that Gmc. *ē<sub>2</sub>* is never written *ei*.<sup>66</sup> He notes also

<sup>65</sup> Collection of data, Hirt, "Zur gotischen Lautlehre", *PBB*, XXI (1896), pp. 159-161. In spite of his precautions (cf. p. 159), errors are numerous. L 2,5; L 5,11; L 14,15; L 19,22; k 6,3 B; Sk. 6,21; Sk. VII,14. Since he mentions the erasure in L 8,23, he should also mention L 9,13; J 11,31; J 11,33, where erasures also occur (cf. Streitberg, *Bibel*). It is not sound practice to mention *ei* for *e* in Biblical names along with the Gothic facts, since they are not on the same level (cf. Gaebeler, p. 8f.).

<sup>66</sup> Bethge, # 28.

that *hue* occurs 45 times, *þe* 165 times, *swe* 384 times, *unte* 373 times, with no single occurrence of *ei*. The fact that  $\bar{e}_2$  is never written *ei* is not significant, since *jer* (ca. 20 times), *redan* (ca. 10 times), *sels*, *selei* (ca. 15 times), *wens* (ca. 20 times), etc., etc. are never written *ei* either. The change seems to be bound to certain words and morphemes. Several attempts have been made to explain this error. Hirt thought that it was conditioned in part by *ei*, *i*, *j* in the next syllable.<sup>67</sup> Kock thought it was a following *-k* — *ei*, *i*, *j* and/or unaccented position which caused *e* to become *ei*.<sup>68</sup> In the 5th and 6th editions of his *Elementarbuch*, Streitberg accepted Sievers' theory that it was the "höher Steigton" which caused *e* to become *ei*.<sup>69</sup> I do not think that we are in any position to tell whether this is a conditioned change, or whether all *e*'s and *ei*'s have fallen together in the scribes' dialect(s). It must be stressed, however, that this "error" can tell us nothing about Wulfila's pronunciation, since it pertains only to the language of the scribes.

3.13.1.b *e* for *ei*.<sup>70</sup> This error occurs 28 times: L 20,10 *gebe<sup>t</sup>na*; Mc 7,33 *spewands*; K 14,26 *skerein*. This phenomenon has not received as much attention from Gothic scholars as the preceding one. Sievers, cited by Streitberg, believes that "tiefer Fallton" caused *ei* to become *e*.<sup>71</sup>

3.13.1.c *i* for *e*.<sup>72</sup> This error occurs 20 times: T 4,1 B *spidistaim* (A right); L 17,6 *qīþeiþ*; Mc. 6,53 *duatsniwun*, etc.

3.13.1.d *e* for *i*.<sup>73</sup> This error occurs 14 times: L 8,17 *swekunþamma*; L 1,29 *hueleika*; Mc. 5,10 *usdrebi*, etc.

3.13.1.e *i* for *ei*.<sup>74</sup> Occurs 5 times: M 27,3 *silubrinaize*; J 12,26 *andbahtiþ*; L 6,40 *laisaris*; T 1,4 A *wisandin* (B right); Sk. VII, 11 *wisandin*.

3.13.1.f *ei* for *i*.<sup>75</sup> Occurs 4 times: Mc. 9,18 *usdreibeina*; J 17,23 *kunnei*; k 4,1 A *andbahtei* (B right); T 3,4 *anawiljein*.

<sup>67</sup> "Zur gotischen Lautlehre".

<sup>68</sup> A. Kock, "Lautgesetzlicher Übergang von *e* zu *ei* im Gotischen vor *k* -*i*, *ei*, *j*", *ZvS*, XXXVI (1900), p. 583; "Ein Beitrag zur gotischen Lautlehre", *IF*, XXX (1912), pp. 244-251.

<sup>69</sup> Streitberg, *Gotisches Elementarbuch*, 5th and 6th editions (Heidelberg, 1920), p. 48f.

<sup>70</sup> Collection of data, Hirt, "Zur got. Lautlehre". The following should be added: M 5,32; L 2,26; L 8,13; L 8,15; L 18,12; L 20,10 *gebeina*; Mc. 9,1; K 8,11 A; k 12,13 AB; T 1,16 B. Braun (cited by Streitberg, *Bibel*, p. 483, but not in the notes at the bottom of the page) read *andabeit* k 2,6 A where other editors had read *-bet*; according to Braun E 1,14 A has *izei*, not *ize*. Two of Hirt's examples are spurious: Th 4,5 AB; G 2,14 B, or at least they are not cited by the editors.

<sup>71</sup> Cf. *supra*, note 69.

<sup>72</sup> Materials collected, Hirt, "Zur got. Lautlehre". The following should be added: L 6,11; L 8,33; L 8,56; L 17,6; G 4,11; Tit. 1,13 A; C 3,15. Hirt has miswritten L 9,20 for L 9,21.

<sup>73</sup> Collected in Streitberg, # 22. The following should be added: L 8, 3; L 14,29; L 20,26; k 3,7 B; E 2,3; G 1,24; G 4,11; th 1,10.

<sup>74</sup> Collected in Streitberg, # 22.

<sup>75</sup> Collected *loc. cit.* T 3,4 should be added.

These examples show that *e* and *ei* had fallen together, and the last two show that *ei* and *i* had fallen together in unaccented position. It is quite possible that *e/ei/i* had fallen together, although we do not have enough examples to state this categorically.

### 3.13.2 *u/o confused*<sup>76</sup>

3.13.2.a *u* for *o*. This error occurs only three times: Mc. 9,50 *supuda* for *-oda*; Mc. 11,32 *uhtedun*; E 1,13 *sunjus* (B right).

3.13.2.b *o* for *u*. This occurs 10 times: Mc 3,17 *sunjos* corrected to *-jus*; E 2,19 AB *aljakonjai*; Mc 10,23 *faiho*, etc.

These errors are usually thought to indicate that *o* was “geschlossenes, dem *ū* nahestehendes *ō*”.<sup>77</sup> If *u* and *o* represented two different “Laute”, two phonemes, in the language of the scribes, it is difficult to see how the phonetic similarity of the two could cause them to be confused. The French *e* is very close to [i] to American ears, but it is never confused by Frenchmen with *i* in orthography or speech. If this confusion of symbols has any meaning to us, phonetically speaking, it can only mean that the two sound types have fallen together.

### 3.13.3 *au/u confused*

3.13.3.a *au* for *u*.<sup>78</sup> Leaving aside the variation in the writing of the sign for the vocative singular of the u-declension, this error occurs 21 times: L 4,3 *sunaus*; E 5,5 *skalkinassaus*, etc.

3.13.3.b *u* for *au*.<sup>79</sup> Leaving aside the variation in the writing of the sign for the vocative singular of the u-declension, this error occurs 14 times: E 4,13 *sunus*; L 1,79 *daupus*; L 4,17 *prafetus*, etc.

It is often affirmed that this variation in writing shows *au* to have been something like [ō].<sup>80</sup> This is denied by others, who feel that this is merely an indication “für die beginnende Unsicherheit in der Flexion dieser Klasse”.<sup>81</sup> Streitberg seems to have held the first opinion in the 3rd and 4th edition of his *Elementarbuch*, but in the 5th and 6th, he follows Sievers in believing that it is the intonation which determines the

<sup>76</sup> Collected Streitberg, # 23. Mc. 3,17 should be added to the examples of *o* for *u*.

<sup>77</sup> Braune-Helm, p. 9.

<sup>78</sup> Collected Streitberg, # 24 A. L 9,11 *leikinassaus* for *-sus* and k 11, 33 *baurgswaddjau* for *-ju* should be added.

<sup>79</sup> Collected Streitberg, # 24 B. T 2,9 A *gal<sup>u</sup>ubaim*; R 9,21 A *galubamma*; T 2,9 B *galubaim*; J 7,41 *pu* for *pau*; M 26,64 *ufto* for *afto*; E 1,1 B *apaustulus* (A right) should be added. Also L 5,6 *dishnaupnodedun* with *a* erased.

<sup>80</sup> Hirt, “Grammatische Miscellen”, *PBB*, XVIII (1894), 280 footnote 1, gives this as the phonetic shape of *au* in endings.

<sup>81</sup> Cf. Braune-Helm, p. 61. The literature on this question is too large to be given here, cf. the note *loc. cit.* and Mossé.

choice of *au* or *u*.<sup>82</sup> The only phonetic fact which can be gleaned from the variation is, in my opinion, that *u* and *au* have fallen together in the language of the scribes.

The reader will not fail to have noticed that there is a discrepancy in the assumption of a falling together of *o* and *u* and *au* and *u*. If this assumption is true, we should expect to find *au* written for *o* and *o* written for *au*. This does not occur. This has not been brought up in previous studies. It may be that we can assume that this would have occurred if our corpus had been larger.

### 3.13.4 *The loss, introduction, and assimilation of h*<sup>83</sup>

3.13.4.a *Loss*.<sup>84</sup> This phenomenon occurs 14 times. Since no count exists, I give the full material: Mc 15,38 *als* for *alhs*; k 9,9 *uswaurts* for *uswaurhts*; Sk. IV, 10 *huarjano* for *-oh*; Sk. V, 22; M 5,15; J 6,11; J 6,11 in the Skeireins (considered by Bernhardt to be introduction of *h*, cf. p. 53 in his *Vulfila*); L 6,12; L 6,17; L 8,4; Mc 15,6; E 1,18 AB; E 5,5 B; G 5,3 B.

3.13.4.b *Introduction*.<sup>85</sup> Occurs 3 (4?) times: L 1,63 *gahmelida*; R 11,17 A *waurhtai*; E 3,13 AB *gawaurhtai*; Mc 10,22 *gahnipnands* (?) (cf. Streitberg's note, *Bibel*).

3.13.4.c *Assimilation*.<sup>86</sup> Assimilation of *h* to *k*, *s*, *b*, *þ*, *d*, *g*, *m*, *n*, *l*, *r* occurs in ca. 130 instances in our material: K 7,16 *nuk-kant*; L 20,16 *nis-sijai*; K 10,21 A *jab-biudis*; M 6,7 *bidjandansuþ-þan*; k 2,16 A *jad-du*; R 14,13 Car. *jag-gatraua*; Ph 3,17 A *jam-mundop*; M 25,42 *jan-ni*; k 1,8 A *jal-liban*; k 8,10 A *jar-ragin*, etc. It should be noticed that, except for the isolated instance *nuk-kant*, *h* is not assimilated to voiceless stops.<sup>87</sup> As mentioned above, most of our examples come from hand III of Ambr. A.<sup>88</sup> An unusual assimilation is seen in L 18,25 *þairþleiþan* for *þairh-*; this may be dissimilation.

There are numerous theories as to the reason for these phenomena. Janko, Streitberg, Moulton, etc. believe that the reason for the loss and the assimilation was a "weak" articulation of the *h*, that *h* signified a glottal spirant.<sup>89</sup> Jacobsohn pointed out, without considering the sound value of *h*, that the assimilation took place only after a weakly accented word, an enclitic.<sup>90</sup> De Vries believed that the loss of *h* could

<sup>82</sup> In the 3rd and 4th edition, p. 49, he says: "Vgl. die Vermutungen Hirts *PBB.* 18,280<sup>1</sup> über den Lautwert dieses mit *u* wechselnden *au*." But in the 5th and 6th edition, p. 50, he gives Sievers' theory.

<sup>83</sup> Cf. Streitberg, # 26; Bernhardt, *Vulfila*, liii; Janko, "Über die Aussprache des gotischen *h*", *Prager deutsche Studien*, VIII (1908), 66; H. Jacobsohn, "Zum gotischen Satzsandhi", XLIX (1920), 132ff.

<sup>84</sup> Cf. Streitberg, # 27.

<sup>85</sup> *Loc. cit.*

<sup>86</sup> Examples given Bernhardt, *Vulfila*, liii, Streitberg, # 26, and Jacobsohn, 132ff. These do not attempt to be complete. A collection of the complete material was made for this thesis, but, since there are ca. 130 examples, it was thought to be too space-consuming to give this here.

<sup>87</sup> *Nuk-kannt* was read by Cromhout as *auk-kannt*, cf. Streitberg, *Bibel*.

<sup>88</sup> *Supra*, 3.05.

<sup>89</sup> Janko, "Got. *h*", Streitberg, p. 59; Moulton, "Stops and Spirants", 7f.

<sup>90</sup> Jacobsohn, "Satzsandhi", p. 132ff. Cf. also Koppitz, "Gotische Wortstellung", *ZfdPh*, XXXIII (1901), 26ff.

be merely a scribal oversight.<sup>91</sup> Jellinek points out that the loss of *h* probably occurred only in the dialect of the scribes,<sup>92</sup> but says later that “Es ist möglich, dass in Wulfilas Sprache *h* überall einen Hauchlaut bezeichnete”.<sup>93</sup> A. M. Sturtevant objects that Gothic *h* could not be a glottal spirant, since the West Germanic etyma show a velar spirant as a reflex of *h* in some positions.<sup>94</sup> As is pointed out by the chart in Pike’s *Phonemics*, p. 70,<sup>95</sup> *h* is the most commonly assimilated of all sounds. Doubtless the assimilation points to a value as a glottal spirant, and this is supported by its loss before consonants and finally. If *h* signified a velar fricative, it was assimilated in a startling manner. It should be remembered, however, that this is not to say that Wulfila pronounced *h* as a glottal spirant, since “errors” in the manuscripts can be interpreted only as concerning the language of the scribe.

### 3.13.5 *Loss and introduction of final -s*

Unfortunately, this phenomenon has not previously drawn the attention of scholars, though the facts are plain.

3.13.5.a *Loss of final -s*<sup>96</sup> — Occurs 5 times: k 7,7 A *gaþrafstip*<sup>9</sup>; C 3,12 B *bleiþein* for *-eins*; k 9,14 B *ufarassau* for *-aus*; t 3,16 *gudiskaizo* for *-zos*; K 10,28 *þuhta* for *-aus*, perhaps because the scribe’s eye strayed to the end of verse 29 (cf. Streitberg, *Bibel*).

3.13.5.b *Introduction of final -s* — Occurs 5 times: k 4,4 A *liuhadeins* (B right); k 6,16 AB *alhs* (perhaps not an error, cf. Streitberg, *Bibel*); Sk. VIII, 17 *baitreins*; Sk. II,25 *garehsnais*; Th 4,3 B *kalkinassaus*.

There is no reason to suppose that these spellings do not indicate an actual loss, especially when we note the loss of the nominative *-s* in the deeds.<sup>97</sup>

### 3.14 *Isolated errors of various types*

There will always be a residue of errors in a manuscript for which we have no explanation, and which, for this reason, give us no information as to pronunciation. They are added here for the sake of completeness: K 9,22 *ummahteigam* for *unm-*, the only example for this type of assimilation; K 16,13 B *gaþwastidai*, with the *ai* partly shaved off; T 4,3 B *gaþarbamante* for *gabarban mate* (A); C 4,10 AB *bi* for *du*; th 1,10 *qemi* for *qimiþ*; Ph 3,13 B *þau* for *nauh* (A); E 2,3 B *hatize* for *hatizis* (A); L 9,8 *sumaiup*

<sup>91</sup> J. de Vries, “De uitspraak der Gotische *h*”, *TNT*, XLIX (1930), 209.

<sup>92</sup> Jellinek, “Germ. g”, *PBB*, XV (1891), 277f.

<sup>93</sup> Jellinek, p. 37.

<sup>94</sup> Cf. *supra*, Chapter I, p. 13, footnote 6. Streitberg indicated, however, that West Germanic *h* was also a glottal spirant, “blosser Hauch”. As Jellinek points out, there is good reason to suppose that OHG *-h* (from *\*-k*) caused *-h* (from *\*-h*) to be “verstärkt”, since the two fall together in OHG (Jellinek, p. 37).

<sup>95</sup> K. L. Pike, *Phonemics* (Ann Arbor, 1947).

<sup>96</sup> Cf. *supra*, 3.04, and 3.12.4.

<sup>97</sup> Cf. Fr. Kluge, “Urkundengotisch”, *Pauls Grundriss*, I<sup>2</sup>, p. 515.



for *-aiþ* (?); Sk. III, 17 *munandane* for *-andans*; Dk. II, 9 *in þiz/ei* for *in þiz/ei*; Neh. 5,18 *gaitsa* for *gaits ·a·*; K 11,3 A *hwiljaup-þan* for *wil-*. Bernhardt notices the *h*, but thinks that it is a number (8) — one of the canons — which has merely been set over into the text. As he remarks, this is strange, if true, since we find *e* = 5 later on.

### 3.2 SYLLABIFICATION

The criterion of word-breaking or syllabification, though it does not concern scribal deviants, must also be discussed here, since it has to do only with the language of the scribes. We find that the striving for uniformity in the length of lines, especially in CA, has brought with it a necessity to break up long words occurring at the end of the line. This practice, which was first seen as a means of determining pronunciation by Gabelentz and Löbe, has received detailed treatment by Hechtenberg-Collitz and Schulze.<sup>98</sup> It is necessary only to repeat their findings here.

Since the word-breaking was presumably done by the scribes and not by Wulfila,<sup>99</sup> we cannot use this criterion for determining Wulfila's pronunciation.<sup>100</sup> We can only use it as a means of determining points of Wulfila's pronunciation if we assume that the scribes are using rules laid down by Wulfila himself.

In our study of syllabification in Gothic, we must remember that, if this criterion is to be used to determine pronunciation, we cannot allow any preconceived notions as to pronunciation to creep in. Thus, when Hechtenberg-Collitz says: "If two vowels follow each other immediately separation takes place ..." and "If two vowels constitute a diphthong, they are not divided ...",<sup>101</sup> she is operating with preconceived notions, and she prejudices her case from the start. In operating with syllabification, we should remember that our task is first to study the graphic signs, and then to try to ascertain what their breaking can tell us.

Since the practice of word-breaking is regular in all the Gothic manuscripts, it might be advanced that it can tell us little about the actual pronunciation of Gothic. One might think that the scribes operated with purely mechanical rules. We cannot rule out this possibility, in spite of Schulze's remarks to the contrary. Yet all possible criteria must be explored.

#### 3.21 *Diphthong vs. monophthong*

It seems logical that we should consider those vowels which are placed in different

<sup>98</sup> Klara Hechtenberg-Collitz, "Syllabication in Gothic", *JEGPh*, VI (1966), 72-86; W. Schulze, "Wortbrechung in den gotischen Handschriften", *BSB*, I (1908), 610-624; W. Schulze, "Zu dem Aufsatz 'Wortbrechung in den got. Handschriften'", *ZvS*, XLII (1909), 327-329.


<sup>99</sup> There is some evidence that Wulfila's autograph was written in the *stichedon* style common in the 5th century. Since Wulfila was a *lector* himself, we might expect this to be true. See *supra* 1.0, footnote 5; 1.01. Since our mss. are, for the most part, not written in this style, it is not likely that the syllabification found in our mss. was done by Wulfila.

<sup>100</sup> Schulze expressly states, p. 489, that the word-breakings give us an insight into Wulfila's pronunciation, without attempting to give reasons.

<sup>101</sup> Hechtenberg-Collitz, p. 72f.

syllables when they follow one another to be either diphthongs or in hiatus.<sup>102</sup> If we find that *ai* and *au* are never divided in our mss., while *aw*, *aiw*, *iu*, *aei* are divided, we must assume that *ai* and *au* had a different syllabic structure than *aw*, *aiw*, *iu*, *aei*. We have seen above that *aei* was used to render Greek *at* in Biblical names.<sup>103</sup> If we note that it is broken in such words as *sa/ei* (J 8,54), we may think that this breaking signals that this (former) compound was still felt to be a compound, and thus was broken in this manner. This is proven to be false by such breakings as *þa/tei* (M 5,27).<sup>104</sup> This may be taken to indicate that *ei* did not have some (phonetic) juncture-marker in the word *saei*. This makes it quite probable that *aei* was a diphthong like *iu*, and that *ai* and *au* were monophthongs. It might be objected that one occurrence of *i/u* (L 15,4 *ni/un*) should not be taken to indicate that *iu* was a diphthong or two vowels in hiatus, and that the lack of a division *a/i* or *a/u* could be merely fortuitous. *ai* and *au* are at least 10 times as frequent in our text as *iu* is.<sup>105</sup> That we do not find them divided even one time cannot be considered fortuitous. This is another indication that *ai* and *au* are monophthongs in Gothic. It should be remembered that this holds true for the dialect of the scribes only, unless we assume with Schulze that the rules were drawn up by Wulfila.<sup>106</sup>

### 3.22

Hechtenberg-Collitz has pointed out that  is treated as a single consonant in word-breaking (cf. *sai/huiþ* Mc. 8,15, *ai/huatundjai* Mc. 12,2 6), and that this perhaps indicates that it is a “simple sound”.<sup>107</sup> This cannot be taken seriously. Since clusters of two consonants are always divided, it would have been necessary for Wulfila to devise a method of dividing the sign (presumably he would have used *h/w*). The fact that Latin *qu* is treated as a simple consonant in word-breaking may perhaps be taken to indicate that *it* represented a close-knit sequence of sounds, since a division *q/u* was possible. Since Wulfila had already used *hw* to mark some kind of juncture phenomenon,<sup>108</sup> he could not use it in word-breaking; nor could the scribes use it in word-breaking unless they wished to depart from the Wulfilian tradition.

### 3.23 *ggw*

We find *ggw* broken some 10 or 11 times in our manuscripts. It is always broken,

<sup>102</sup> Hechtenberg-Collitz, p. 72f., does not believe that this can indicate a diphthongal pronunciation, but Schulze (“Wortbrechung”, p. 496) seems to think this is probable. Jellinek, p. 47, says: “Nicht zu einer Silbe gehörige Vokale und Diphtonge werden getrennt ...” We cannot be sure of his meaning, but his examples indicate that he believes this *may* indicate diphthongal pronunciation.

<sup>103</sup> *Supra*, 2.11.21.

<sup>104</sup> Hechtenberg-Collitz, p. 172.

<sup>105</sup> *ai* and *au* have a combined list-frequency of over 1500, whereas *iu* has a list-frequency of ca. 100.

<sup>106</sup> Schulze, “Wortbrechung”, p. 489f.

<sup>107</sup> Hechtenberg-Collitz, p. 174, footnote. This viewpoint has been widely accepted, cf. Braune-Helm, p. 38.

<sup>108</sup> Cf. Braune-Helm, p. 38.

except for one time, in the following manner: *usbligg/wandans* (L 18,33), *usugg/wuþ* (Mc. 12,20), *trigg/wos* (C 4,7), *sigg/wada* (k 3,15). We find only one exception: *trig/gwa* (Th 5,27). This latter might be taken to indicate a pronunciation as a stop plus stop plus *w*. If we accept this, however, we must think that *hug/greip* (J 6,35) also indicates a geminate stop plus *r*. Since a number of such cases come up, we cannot accept this one exception as indicating a pronunciation.<sup>109</sup> The word-breaking points to a uniform pronunciation for all cases of *ggw*.

### 3.3 CONCLUSIONS

The value of having the entirety of the evidence for scribal errors, here gathered for the first time, cannot be stressed too heavily. Errors which at first seem important are put into their proper perspective. The value of what is called in NT criticism “internal evidence of documents” cannot be overestimated. When all the errors are gathered and sifted, very little remains which can give us an idea of the pronunciation of the scribes. This perhaps can be laid to the account of the paucity of material. It is evident, however, that previous scholars have been overly optimistic about what scribal errors can tell us. The only satisfactory evidence we have is for the falling together of *ei* and *e*. All the rest is merely guesswork, using the hints the errors give us. And even when we accept the evidence at face value, it tells us little, if anything, about Wulfila’s pronunciation.

Scribal errors reach back certainly into the 5th century, however; we find several in Car., and the *Vorlage* of A and B seems to have contained several also (cf. T 2,9 A *gal<sup>u</sup>ubaim*, B *galubaim*). A closer inspection of the Veronese marginal notes might reveal much in the way of comparisons with CA, thus affording us information to use in reconstructing the *Vorlage*. At the present stage of the game, it is best to consider all errors to be of the late 5th or early 6th century and, hence, non-Wulfilian.

The results of this chapter must be, then, negative. The criteria of scribal errors and of word-breaking, considered by most previous investigators to afford us direct evidence of Wulfila’s pronunciation, have been shown to pertain only to the pronunciation of the scribes. Before these criteria can be used at all, we need to have more information about the scribes’ dialect(s) and its relation to that spoken by Wulfila, for we can use it as evidence only if we assume that it is a later stage of Wulfila’s own dialect or that Ostrogothic and Visigothic had the same phonic structure. Since we are in no position to ascertain either of these points, we cannot use the evidence presented in this chapter to determine anything about Wulfila’s pronunciation directly. It should be treated as a part of the comparative evidence (Chapter 5). The fact that we know next to nothing about the language of the scribes makes it difficult to use even there, and it is best left aside entirely in a discussion of the pronunciation of Wulfila’s Gothic.

<sup>109</sup> A number of such cases are discussed by Schulze, “Wortbrechung”, p. 486.

## INTERNAL EVIDENCE II: SYSTEM-DERIVED EVIDENCE

In the previous chapter, we discussed the evidence afforded by deviations from the spelling system; in the present chapter, the evidence to be derived from a consideration of the sound system itself is discussed. This type of evidence is properly divided into two parts: (a) regular alternations within the system; (b) the configuration of the system itself. The reason for this division is self-evident; the two types of evidence are of unequal value in determining the phonetic values of Wulfila's signs. Criterion (a) gives evidence concerning a previous stage of the language; criterion (b) gives evidence concerning the language of the person composing the texts we have. A discussion of these points will be found in the proper place below.

## 4.1 EVIDENCE DERIVED FROM THE CONFIGURATION OF THE SYSTEM

4.10 *Methodological remarks*

By configuration of the system, I mean those things which would usually appear in a discussion of the phonemics of a language: patterning, distribution, juncture (or border point, cf. Pike, *Phonemics*, p. 179a), etc. These things have not received the attention of many scholars. In the first volume of *TCLP*, Trubetzkoy devoted one page to the vowel system of Gothic.<sup>1</sup> By bringing his theory of "Intensitätsvorstellungen" and "Intensitätssystem" to bear, he thought he could demonstrate that *ai* and *au* represented two phonemes each in Gothic. To do this, he must insist that "Obgleich objectiv 'au' ein Diphthong und 'o' ein Monophthong waren, wurde das Verhältnis 'au:o' (bzw. 'ai:e') im phonologischen Sprachbewusstsein etwa mit dem Verhältnis 'ū:u' oder 'ā:a' gleichgestellt ..." I do not see how we can call a diphthong a long correspondent of a long vowel (Trubetzkoy considers *o* to be a long vowel). Heffner has pointed out that the method of minimal pairs cannot be used for Gothic, since our corpus is so limited.<sup>2</sup> In his two articles on Gothic phonemes, Moulton has used both patterning and distribution to some extent, but he has failed to provide us with a discussion of the methodological bases for this.<sup>3</sup> Jelinek used patterning in

<sup>1</sup> "Zur allgemeinen Theorie der phonologischen Vokalsysteme", *TCLP*, I, p. 57.

<sup>2</sup> Heffner, "A Note on Phonological Oppositions".

<sup>3</sup> Cf. "Phonemes", footnote 28; "Stops and Spirants", *passim*.

his discussion of the pronunciation of *w*, when he pointed out that *w* was kept apart from *b* and *f* (which he assumed to be labial spirants), so it could not be a labial spirant.<sup>4</sup> He assumed a labial order *p*, *b*, *f*, *w*, *m*, each contrasting with the other. This unfortunately appears to be the sum total of considerations of the configuration of the system in previous studies of Gothic.

If we were able to use such evidence in determining the pronunciation of Gothic, it would constitute our only immediate source for Wulfila's pronunciation. If we set aside the scribal errors, we must assume that the language we have written down in our manuscripts is that of Wulfila himself; and the system in which it is written down is that of Wulfila himself. If a phonemic opposition obtains in our material, we must assume that this opposition obtained in Wulfila's dialect (or idiolect). Evidence derived from the configuration of the system is witness to Wulfila's pronunciation.

Since the division in this part of the chapter is of necessity different from that in the second part, a general theoretical discussion cannot be given here. Each criterion is discussed from the point of view of what it can tell us in the appropriate section below.

#### 4.11 *Patterning*

Things which pattern alike share some feature in common. Thus, if we find that *p*, *t*, *k* pattern alike in a language, we assume that they share the feature of stop articulation, or of voiceless articulation. Sound systems tend to be symmetrical.<sup>5</sup> An assumed pronunciation or pronunciation type which makes the system symmetrical is to be preferred, *ceteris paribus*, to one which does not do so. When it is a question of deciding whether a particular sequence of sounds is a cluster or a close-knit sequence, it is necessary to observe the patterning of other clusters in the language, since "Characteristic sequences of sounds exert structural pressure on the phonemic interpretation of suspicious segments or suspicious sequences of segments".<sup>6</sup>

##### 4.11.1 *Patterning of q and hu*

As Moulton has pointed out, clusters of apical consonant plus *w* are frequent in Gothic: *twalif* (Mc 6,7), *þwahan* (J 9,7), *dwala* (M 5,22), *swa* (M 5,16), *izwis* (M 5,18). Labial consonant plus *w* does not occur. *gw* occurs only once in one word, *bidagwa* (J 9,8),<sup>7</sup> but *ngw* (*ggw*) occurs often (*siggwan* L 4,16). Moulton thought that this must indicate that *q* (which we know has a labio-velar articulation, cf. 2.32.g) represented a sequence of *k* plus *w*, which otherwise would not exist and whose lack would constitute a hole in the symmetry. The same may be said of *hu*.

<sup>4</sup> Jellinek, "Gotisch *w*", *Zfda*, XXXVI (1893), p. 267.

<sup>5</sup> Cf. Pike, *Phonemics*, p. 59 and *passim*.

<sup>6</sup> *Ibid.*, p. 60.

<sup>7</sup> Cf. Moulton, "Phonemes", footnote 28.

4.11.2 *Patterning of ggw*

It is commonly assumed that the compound graph *ggw* had two values in Gothic; that it signaled a velar nasal plus stop plus *w* in *siggwan* and *aggwus*, but a geminate *g* plus *w* in *bliggwan*, *triggws*, *glaggwo* and *skuggwa*.<sup>8</sup> If we allow *ggw* to represent a geminate *g* plus a *w*, we find that is contrary to the patterning of other clusters. Clusters of four consonants in word final position occur in 78 lexical items in Gothic, a total of 250 times, always containing a liquid or a nasal.<sup>9</sup> Clusters of four which do not contain a liquid or nasal do not occur in final position. The cluster *ggws* occurs in two lexical items a total of 11 times in final position. Structural similarity seems to point to the existence of a nasal in this cluster. This is, of course, supported by the fact that *gg* represents velar nasal plus *g* in Biblical names (2.32.ba).

4.11.3 *Consonant length or gemination*

*p*, *t* (cf. section 4.21.b), *k*, *f*, *h*, *b*, *z* do not occur in geminate clusters in Gothic, except that *pp*, *tt*, *kk* occur in loanwords. *þþ* occurs in three words and in words in which *h* has been assimilated to a following *þ* (cf. section 3.13.4.c). *m*, *n*, *l*, *r*, *g*, *d* occur geminated in native Gothic words; *gg* and *dd* occur only in the clusters *ggw* (if we accept the usual interpretation) and *ddj*. This indicates that *dd* and *gg* (*ddj* and *ggw*) had an unusual status in the Gothic sound system.

4.12 *Phonemic opposition*

There may be said to be two types of phonemic opposition, performed by the linguist: the opposition of two sounds to see if they are in contrast,<sup>10</sup> and the attempt to ascertain which features mark the contrasts which obtain in the language. The latter type of opposition has received much attention of late, and has been discussed in detail in the literature.<sup>11</sup> To all practical purposes our discussion must center around correlative distinctions, around correlative pairs and correlative sets or correlations.<sup>12</sup> It is best to operate with orders and series. If all the phonemes within an order or a series are distinct from one another, as they must be, there must be some mark of this

<sup>8</sup> Indeed I know of no scholar who questions this. A citation of authorities is therefore not necessary. For a discussion of the reasons for this assumption, see section 5.11.2.bb.

<sup>9</sup> The figures are taken from an unpublished dissertation by M. Joos, University of Wisconsin, 1940.

<sup>10</sup> Cf. Heffner, "Phonologic Oppositions", who calls this confrontation of sounds (i.e. the discovery of "minimal pairs") "phonologic opposition". Mossé calls it (in his bibliography, p. 282) "the method of phonological opposition".

<sup>11</sup> This type of opposition was discovered by Jakobson (cf. Trubetzkoy, *Principes*, p. 89) and has remained to this day an almost exclusive property of the Prague school. For a discussion of the older literature, cf. Trubetzkoy, *Principes de Phonologie*, trans. by Cantineau (Paris, 1949), p. 87 ff. For a recent discussion, see Jakobson, Fant, Halle, *Preliminaries to Speech Analysis* (Cambridge, 1952).

<sup>12</sup> For a definition of the term correlative and the term correlation, see Trubetzkoy, *Principes*, p. 89. See further A. Martinet, "Rôle de la corrélation dans la phonologie diachronique", *TCLP*, VIII (1938).

distinction, some feature of articulation, which keeps them apart. It would seem that it would not be difficult to discover in each case what this feature was in Gothic. Such is not the case. If we assume *b* and *p* to be a correlative pair, it may be a lenis versus a fortis articulation which keeps the two apart, along with other correlative pairs of the sort (other correlations), *d* and *t*, *z* and *s*, *g* and *k*. Yet one could equally well maintain that it is voiced versus voiceless articulation which keeps them apart, since they apparently possess both the opposition lenis vs. fortis and the opposition voiced vs. voiceless articulation. If we can determine which is the “primary” distinctive feature, as Moulton has attempted to do,<sup>13</sup> of the opposition *p:f:b*, for example, this will be something. Our discussion of such features must not be colored by our assumed pronunciation of these signs, however; we cannot make a componential analysis of Gothic phonemes.

If we sort Gothic signs into series and orders according to the sounds they apparently signal, according to the above discussions, we find the following pattern:

<i>Consonants</i>		
Labials	Apicals	Velars
p	t	k q (?)
f	þ	(h)
b	d	g
m	n	ŋ (written g)
w	j	(hu)
	s	
	z	
<i>Vowels</i>		
Front	Central	Back
ei		(iu)
i		u
e		o
ai		au
	a	

The question is to determine what is the mark of distinction in each case.

#### 4.12.1 *Consonants*

The orders are distinguished from each other by the place of articulation.

4.12.1.a *p:f:b, t:þ:d* — In the labial and the apical orders, we find a three-way contrast between *p:f:b* on the one hand and *t:þ:d* on the other. *p* and *f*, *t* and *þ* are in contrast in all positions except before *t* (cf. section 4.21.b), whereas *f* and *b* and *þ* and *d*, respectively, are not in contrast in final position after vowels and before *-s*. Since *p* and *b* and *t* and *d* thus do not contrast in final postvocalic position (*b*

<sup>13</sup> “Stops and Spirants”, p. 5f.

and *d* do not occur in this position), and *b* and *d* are thought to have both stop and spirant allophones, Moulton has been led to conclude that the primary feature which distinguished *p:f:b*, *t:þ:d* was voice, which distinguished voiced *b* from voiceless *p* and *f* and voiced *d* from voiceless *t* and *þ*.<sup>14</sup> But if we are not willing, as some have not been (cf. section 4.20 and section 4.21.a), to allow *b* and *d* to have spirant allophones, the primary distinction is one of stop vs. spirant (*p:b*):*f*. Or if we deny that *b* and *d* may have stop allophones, then the opposition is again one of stop vs. spirant, but in a different way: (*b:f*):*p*. If we accept the spirant allophones of *b*, we must accept Moulton's conclusions. If *f* is labio-dental, however, as some have thought, then the opposition is one of place of articulation (*p:b*):*f* (or, if we believe that *b* signaled a labio-dental: (*b:f*):*p*).<sup>15</sup> This method can tell us nothing about Wulfila's pronunciation of *p*, *f*, *b*, *t*, *þ*, *d*, since we need to know this pronunciation to use the method.

4.12.1.b *h*. — The evidence definitely points to a point of articulation for *h* which corresponds to that of *k* and *g* (presumably velar). If we allow the labio-dental articulation of *f*, however, an argument of structural similarity loses its force, for the second series on the chart has then one member which is not of the same order as the first series and one which (presumably) is. In fact, we may be led to believe by the other evidence for *h* (cf. *supra*, 3.13.4) that *þ* was pronounced at a different point from *t* and *d*, so the point of articulation for the second series is regularly further back than that for the first series. One could defend the position that the distinction between *p*, *b* and *f*; *t*, *d* and *þ*; *k*, *g* and *h* was primarily one of point of articulation rather than the manner of articulation.

4.12.1.c *Conclusion* — The method of correlative pairs cannot be used to determine the articulatory features of consonants in Wulfilian Gothic.

#### 4.12.2 *Vowels*

If we try to determine which are the features which kept the vowels apart, we cannot go much further than we did with the consonants. It seems evident that the feature which held *i:u*, *e:o*, *ai:au* apart was front vs. back articulation. If we allow umlaut in Proto-Gmc., as some have been willing to do, however, it is quite possible that the difference is not one of back vs. front, but one of lip-rounded vs. lip-neutral or lip-spread.<sup>16</sup> Thus the verb *sokjan* may have been pronounced [sækjan] or the like.

The system is asymmetrical, in that the front position shows four degrees of aperture, whereas the back order shows only three. Such asymmetry is not unusual, however,

<sup>14</sup> "Stops and Spirants", p. 5f.

<sup>15</sup> On the possibility of *f* representing a labio-dental, cf. Jellinek, in a review of Streitberg, 2nd ed., *AfdA*, XXXI (1907), 2 f. This view was originally proposed by Wimmer, who felt that Wulfila did not use Greek *phi* for his *f*-like sound, because his *f* was labio-dental.

<sup>16</sup> See above, 2.31h.



as Martinet has pointed out.<sup>17</sup> Since *ei* patterns like *iu*, and since it never seems to pattern in a set with *u* (save that we find *leikeis* and *brukeis* vs. *lagjis*), we are not justified in assuming another *u*-sound as a correlative of *ei*.

There can be little doubt that the difference in the series is one of aperture. There are probably also differences of place of articulation, as this is very common in languages.

4.12.2.a *Conclusion* — The method of correlations can tell us that Wulfila's Gothic distinguished degrees of aperture in vowels.

#### 4.13 *Method of contrastive pairs*

There is another type of phonemic opposition performed by the linguist, which consists of confronting pairs or sets of pairs of words to discover what phonetic feature distinguishes them from each other. This is the process of discovering minimal pairs or analogous environments. As mentioned above, Heffner has denied that this method can be applied to the problem of the pronunciation of *ai* and *au* in Gothic, since minimal pairs are not forthcoming.<sup>18</sup> If we restrict the "method of phonologic opposition" to minimal pairs, this is certainly true. As Pike has pointed out, contrast in minimal pairs is "merely an instance of contrast in a specific kind of analogous environment".<sup>19</sup> If we restrict our method to minimal pairs alone, we ignore the very useful criterion of contrast in analogous environments. If we can find the same sign occurring in an analogous environment in different words, we must assume that, if a difference in pronunciation existed, it was phonemic, since it distinguished lexical items. The assumption that one sign signals two phonemes is only probable if we assume that Wulfila had no convenient way to signal the phonemic difference. If Wulfila heard the difference between his own phonemes, as he must have, and if the Greek alphabet provided him with a means of signaling this difference, he would be apt to signal the difference by choosing a different symbol for one of the sounds in question.<sup>20</sup>

When we find *þlauh-s* (Mc. 13,8) and *-þlauh* (k 11,33); *nauh* (M 27,63) and *-tauþ* (Mc. 9,2); *-faihoda* (k 12,17) and *faihugairneins* (tit 1,11); *haihamma* (Mc. 9,47 with a Nom. presumably *haihs*) and *saihs* (T 5,9), etc., we must assume either that Wulfila had no way to signal a contrast between these "phonemes", or that there is merely one phoneme present in each pair in Wulfila's Gothic. As has been pointed out above (section 2.21.11), he had a means of signaling a contrast between *ái* and *ai*. It might be objected that *í* is used in Gothic to signal close juncture in such forms as *fraitan* and *usiddja*, and that it could not, therefore, be used to signal a diphthongal pronunciation of *ai* (that is, *aĩ*). As Wrede has pointed out, Greek did not use the points

<sup>17</sup> A. Martinet, "Function, Structure and Sound Change", *Word*, IX (1950), 12-28.

<sup>18</sup> Heffner, "Phonologic Oppositions", *passim*.

<sup>19</sup> Pike, *Phonemics*, p. 81a.

<sup>20</sup> Cf. Mossé, *Manuel de la langue gothique* (Paris, 1942), p. 44ff. For a critique of this view, see Pisani, p. 118.

to signal juncture.<sup>21</sup> We cannot be sure that it was so used in Gothic, since we find it also used to signal diphthongal or disyllabic pronunciation in such forms as *sauil*.<sup>22</sup> We can only be sure that it is used to signal word-initial and syllable-initial, as in Greek. If Wulfila did not want to use *ai* to signal a diphthongal pronunciation, he could use *aei* as he did in Biblical names.<sup>23</sup> Wulfila also possessed a means of signaling the diphthongal pronunciation of *au*, if this existed in his dialect. As Mossé has asserted: "Quand le gothique a à noter une diphthongue étrangère à son système, il n'emploie pas *ai au*, mais *aiw* pour *eu*, *aw* pour *av*, lat, *au*."<sup>24</sup> Pisani is right in taking exception to Mosse's citing of *kawstjon* (Lat. *cautio*), which is a form found in the Naples deed, written ca. 552.<sup>25</sup> This takes nothing away from his argument, however; Wulfila has transcribed Greek diphthongs containing a *u*-colored second component with vowel plus *w*, not vowel plus *u*. I agree with Hirt that Wulfila must have been rather stupid not to use the same device in Gothic if he needed it.<sup>26</sup> This is the most cogent argument for the pronunciation of *ai* and *au* as monophthongs.

## 4.2 REGULAR ALTERNATIONS WITHIN THE SYSTEM

### 4.20 Methodological remarks

In all languages there are changes in the form of morphemes which depend directly upon the phonemic environment. Such changes are commonly called morphophonemic variations to distinguish them from morpheme alternations, in which the choice of the morpheme is determined by the morphemic environment. The evidence we have for morphophonemic variants in Gothic is purely orthographic. It is tacitly assumed by all Gothic scholars that a regular alternation in graphic signs indicates a regular alternation in "sounds", in phonemes.<sup>27</sup> There can be no reason for denying the logic behind this. There must be some reason for an alternation in graphic signs

<sup>21</sup> Wrede, p. xiii. On the Greek practice, see Thompson, p. 63. This fact was first noted in Gabelentz and Löbe (p. 18f.).

<sup>22</sup> Wrede, # 3, note 5. Cf. further Streitberg, "Gotisch *i* und die Verbalkomposita", *IF*, XXIV (1909), 174f.

<sup>23</sup> Cf. *supra*, 2.11.21.

<sup>24</sup> Mossé, *Manuel*, p. 44. It should be pointed out that a diaeresis mark is used over *u* in Greek mss. in the same manner as over *i* (Thompson, p. 63).

<sup>25</sup> Pisani, p. 118.

<sup>26</sup> Hirt, *Handbuch des Urgermanischen*, Vol. I (Heidelberg, 1930), p. 39f. For a rejoinder, see Jellinek, "Gotisch *ai* und *au*", *ZfdA*, LXVI (1929), 117-119. The literature on this question is quite extensive; for the older literature see the article by Jellinek just mentioned. For more recent literature see Mossé, who does not, however, cite discussions which form parts of books not dealing primarily with Gothic.

<sup>27</sup> This is not true of all scholars. W. G. Moulton considered the graph *w* to represent two phonemes, a syllabic [u], a part of the *u*-phoneme and a non-syllabic [ɥ], a part of the *w*-phoneme ("The Phonemes of Gothic", p. 84f.); for a critique of this point, cf. Penzl, "Orthography and Phonemes in Wulfila's Gothic". But even Moulton considers the alternation *b/f* to be an alternation between phonemes. Cf. on this question the remarks of Dietrich, p. 72.

that is carried out with a great degree of regularity. As a sign represents a sound, a change in signs must represent a change in sounds, unless analogy is in play. If we can rule out analogy, as we can in the changes treated below, we can consider that a change in signs indicates a change in sounds.

If there is any one thing about Gothic pronunciation upon which all scholars may be said to agree, that thing is that *b*, *d* represent spirants in at least some positions.<sup>28</sup> The main reason for this, in spite of the fact that loanwords and the alphabet are often cited, lies in the fact that the variation *f/b* (cf. *hlaif/hlaibis*) is thought to indicate that *f* and *b* share a feature of articulation which they do not share with *p*, that *f* and *b* form a correlative pair.<sup>29</sup> Since all evidence points to a value as a voiceless spirant for *f*, *b* is thought to be the corresponding voiced spirant. The most definite statement concerning the use of morphophonemics in determining the pronunciation of Wulfila's Gothic comes from a most unexpected quarter. In Moulton's latest treatment of Gothic, we find the following statement:

However, this alternation (postconsonantal alternation of *b* and *f* in Gothic, due to the effects of Verner's law) is not automatic, as is proved by a minimal contrast such as *þarb* 'needy' vs. *þarf* 'needs'; and *only automatic morphophonemic alternations* (like *giban*, *gaf*) *can be taken as evidence for the phonetics of Gothic*.<sup>30</sup>

In refutation, I should like to quote from an article which appeared in the same journal eight years earlier: "... compulsory alternation between restricted /x/ and free /y/ in a paradigm indicates a *previous* conditioned sound change from /x/ to /y/ in the position from which it is now excluded (*italics mine*)."<sup>31</sup> I should like to add: "and that is all it indicates". I do not believe that any serious student of Gothic would maintain that the *p* of *huopan* signals a spirant pronunciation, yet the alternation *p ~ f* before *t* (cf. *huoftuli*) is no less automatic than the alternation *b ~ f* in word-final position.<sup>32</sup> Or is the *t* of *wait* to be considered a spirant because it alternates with *s* before the *-t* of the second singular present (*wait*, but *waist*)? One might, it is true, point out that the above-mentioned alternations affect only a small part of the lexicon or of the morpheme inventory of the language, whereas *b/f*, *d/þ* affects the whole language. Such a consideration is equally invalid. Given the Skt. paradigm *vāk*, *vācam*, *vācā*, etc., we should have to assume either an affricate pronunciation of the *k* or a stop pronunciation of the *c*. Or given the Sanskrit correspondence *h*:

<sup>28</sup> Except for Sievers (reported by Streitberg, 5th and 6th ed., # 35.8), who believed that *b*, *d* were stops in all positions.

<sup>29</sup> Cf. Jelinek, p. 34: "In got. Wörtern entspricht regulär einem inlautenden *b*, *d* nach Vokal im Auslaut und vor dem -s des Nominativs *f*, *b* ... Daraus schliesst man, dass nach Vokal *b* und *d* stimmhafte Reibelaute bezeichneten, die im Auslaut und vor Nom. -s stimmlos wurden ..." Cf. further Streitberg, p. 61: "Den besten Beweis für ihre spirantische Geltung bildet das Lautgesetz, dass an ihrer Stelle im Auslaut und vor *s* die entsprechenden stimmlosen Spiranten (geschrieben *f*, *þ*) fordert ..."

<sup>30</sup> Moulton, "Stops and Spirants", p. 4, footnote 1 (*italics mine*).

<sup>31</sup> H. M. Hoenigswald, "Sound Change and Linguistic Structure", *Language*, XXII (1946), p. 140.

<sup>32</sup> In other words, we have a morpheme *huop ~ huof*, in which the morphophonemic variant *huof* occurs only before *t*.

final *t*, *h*: final *k*, would we be willing to assume two *h*-phonemes in the face of orthoepic evidence?<sup>33</sup>

It must be seen from the above discussion that we cannot use morphophonemic variations as evidence for Wulfila's pronunciation. Yet we cannot relegate them to the same position as comparative evidence. It is true that they are a type of comparative evidence. Yet, where the usual comparative evidence is both diachronic and diatopic, evidence derived from morphophonemic variations is only diachronic. By using the criterion of Hoenigswald (mentioned above), we can reconstruct an earlier stage of Gothic. This Pre-Gothic must be syntopic by virtue of the fact that it is reconstructed from one dialect alone. But it is not necessarily synchronic, since no one can tell whether the change of *t* to *s* in the prototype of the form *waist* took place at the same time as the change of *p* to *f* in the prototype of *huoftuli*. If we know a feature existed in a previous stage of a dialect, we can be sure that there is more likelihood of its existing at the present stage than there is for the existence of a feature known to exist at a previous stage of another dialect. Taken together with other evidence, morphophonemics is one of our best sources of evidence; it is to be preferred to comparative evidence. In the absence of supporting evidence, its witness may be given high regard. When other evidence is contrary to its witness, it should usually be disregarded. It must always be used with caution.

Internal reconstruction, that is, the use of morphophonemic variations to reconstruct earlier stages of a language, can be of great value to us in another phase of our study, however. It is almost a truism that all languages have dialects, that all languages are diatopic in character. For this reason, we are not privileged to consider Proto-Germanic to have been absolutely without dialectal cleavage. Hence, when we assume that Proto-Germanic contained a certain feature because it is exhibited by North and West Germanic, we are doubtless justified. But when we assume that Gothic must also contain this feature, we are not justified. For what guarantee do we have that that particular dialect of Proto-Germanic from which Gothic issued did not differ at precisely this point from the other dialects of Proto-Germanic? It may be objected that we assume some time of complete unity when we do etymologies and discover cognates. This cannot be denied. Yet we reconstruct item by item, and who is to say that a particular reconstructed form did not originate in one dialect and thence spread to the others? This unity of which we speak may lie beyond the ken of linguistics. No one can maintain that Germanic was a non-dialectal, syntopic language when it split off from Indo-European. It is at this point that internal reconstruction should be introduced.<sup>34</sup> When we use internal reconstruction, we recon-

<sup>33</sup> On the Sanskrit alternation *h/t* and *h/k*, cf. Whitney, *Sanskrit Grammar* (Leipzig, 1879), # 147 and # 223.

<sup>34</sup> For the methodology involved in this operation cf. the following articles: E. Hermann, "Ueber das Rekonstruieren", *ZvS*, XLI (1911), 1-69 (the pioneer article in the field); H. M. Hoenigswald, "Internal Reconstruction", *SIL*, II (1944), 78-87; H. M. Hoenigswald, "Sound Change and Linguistic Structure", *Language*, XXII (1946), 138-143; J. H. Bonfante, "On Reconstruction and the Linguistic Method", *Word*, I (1947), 83-91, 132ff. (covers the older literature). For a practical

struct that dialect of Proto-Germanic from which Gothic issued and no other.<sup>35</sup> Internal reconstruction must be seen as a tool with which we may implement comparative evidence.<sup>36</sup> Without it, comparative evidence must always be used with extreme caution, and its witness must be questioned at every step of the way.

#### 4.21 *The evidence and discussion of the evidence*

The evidence is presented under various headings below, and is discussed under each heading. It would perhaps have been well to preface a statement of the distributions of the graphemes to a discussion of the morphophonemics, but such statements are easily available in the literature, and it would have been superfluous to repeat them here.<sup>37</sup> Wherever a discussion of the distribution is found, it should be understood that I am citing from Jellinek.

##### 4.21.a *Alternation between $b$ , $d$ , $z$ and $f$ , $\bar{p}$ , $s$* <sup>38</sup>

$b$ ,  $d$ ,  $z$  alternate with  $f$ ,  $\bar{p}$ ,  $s$  in word final position, and  $b$  and  $d$  alternate with  $f$  and  $\bar{p}$  before  $s$ . This is usually considered as proof that  $b$ ,  $d$  and  $z$  were spirants in Wulfila's Gothic, at least in position after vowel.<sup>39</sup> As has been pointed out above, this merely proves that  $b$ ,  $d$ , and  $z$  had spirantal allophones in postvocalic position in Pre-Gothic.<sup>40</sup>

The fact that  $g$  is not replaced by  $h$  in final position and before  $s$  has long received the attention of scholars.<sup>41</sup> Structural similarity would seem to force us to believe that  $g$  also was replaced by a voiceless spirant in final position.<sup>42</sup> If this is true, we must interpret  $h$  as a spirant articulated in a position other than that in which  $g$  is articulated, since  $g$  is not replaced by  $h$ . It is then natural that Wulfila did not use another sign to signal this spirantal allophone of  $g$ . It may well be that he was not aware of

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application, see R. A. Fowkes, "Synchronic Method and Welsh Consonantism", *Word*, IX (1950), 142-145.

<sup>35</sup> On the value of such evidence for the reconstruction of protolanguages, see J. Kurylowicz, *L'accentuation des langues indo-européennes* (Cracow, 1952), p. 3ff.

<sup>36</sup> Hoenigswald, p. 138, p. 142.

<sup>37</sup> Cf. the sound statistics in Jellinek, pp. 53-61; complete frequencies of all sounds in Gothic are given in an unpublished dissertation by M. Joos (University of Wisconsin, 1940), an excerpt from which was published in *Language* cf. p. 61, footnote 4. Unfortunately the use of Joos' work is made difficult by his arrangement of it (he cites initial, end of stem and end of word sounds), and by the fact that he has arranged it according to the (supposed) sound of the sign, instead of by the sign itself. The approach of Joos should prove quite useful in studies of a diachronic nature; it was found unfruitful here. Some statistics on the sounds are given in Moulton, "Stops and Spirants".

<sup>38</sup> Cf. Streitberg, # 35; Jellinek, p. 34f.; Braune-Helm, # 79.

<sup>39</sup> Cf. Moulton, "Stops and Spirants", p. 4; Streitberg, # 35; Jellinek, p. 34; Braune-Helm, p. 33.

<sup>40</sup> For the purposes of this chapter, *Pre-Gothic* is used as a cover term for the syntopic dialect from which Gothic emerged. No statement is made about the temporal aspect of this dialect. It is determined in space but not in time.

<sup>41</sup> It is first noted by Gabelentz and Löbe, p. 41f. Cf. further de Vries, p. 212ff., and the literature cited there.

<sup>42</sup> Streitberg, p. 61f.; Moulton, "Phonemes", p. 79, footnote 15; de Vries, p. 212ff.

the allophonic variation, since such variations form what Whorf has called “background phenomena” for native speakers of a language. If we accept this reasoning, we must date the change *g* to *h* before *t* (cf. *mahts*, *mahta* to *mag*; *ohta* to *og*; *bauhta* to *bugjan*, etc.)<sup>43</sup> earlier than the devoicing of final *g*. We must further posit a time when *g* and *h* shared some feature of articulation which they no longer share, presumably velar position.<sup>44</sup> One might well assume, however, that *b*, *d*, *g*, or *g* alone, were stops in all positions in Gothic, and that the writing with *g* was morphophonemic or analogic. The person taking such a point of view could point to such forms as *magt* (whereas *b* and *d* are always replaced by *f*, *s* before *t*). We can only be certain of certain features in Pre-Gothic. We can be certain that, at the time when *\*g* became *\*h* before *\*t*, *\*g* did not become *\*h* in final position, unless some intervening analogy has taken place. This point is important in the discussion of the relative chronology of the change *b* to *f* and *d* to *s*, *þ*.

This change did not take place in post-consonantal position (*þarb*, *lamb*, *band*, *gard*, *gards*). It has been thought that this indicated that Gothic had only stop allophones here.<sup>45</sup> Hench has maintained that the final *b* in *rb*, *lb* was a spirant, on etymological grounds.<sup>46</sup> The only thing of which we can be sure is that *b* and *d* after consonants in final position were considered members of the same phoneme as all other occurrences of *b* and *d*. It may well be that they were spirants and that the liquid or nasal coming before caused them to retain their voice. It may be that all *b*'s and *d*'s were stops in Gothic. At any rate, we may be sure that the change *b* to *f* and *d* to *þ* never took place in such cases, for we have no reason to assume that analogy would work in those cases and in no other.

#### 4.21.b *Alternations before t*

Before *t* only the signs *l*, *r*, *m*, *n*, *f*, *s*, *h* appear, except for the form *magt*, in which analogy is patently at play. *f* replaces *p*, *b* in this position, *s* replaces *t*, *þ*, *d*, and *h* replaces *k* and *g*. *s* perhaps replaces *z*, but such a case is not found in our corpus. This variation seems to point to a time at which *p*, *b*, *f*; *s*, *t*, *þ*, *d*; *h*, *k*, *g*, respectively, shared some feature of articulation, presumably they were of the same order. The fact that neither *q* nor *hu* occurs before *t* is lamentable. This might afford us a clue as to their (previous) phonemic status as unit sounds or clusters. The clusters *pt* and *kt* occur only in loans.<sup>47</sup> The two words with *tt* (*atta* and *skatts*) are also loans.

<sup>43</sup> Cf. Braune-Helm, p. 39f.

<sup>44</sup> Cf. Moulton, “Phonemes”, p. 79.

<sup>45</sup> Cf. Streitberg, # 35.8, Anmerkung 14: “Dass *b* und *d* nach Nasalen und Liquiden einen Verschlusslaut bezeichnen, erhellt aus der Tatsache, dass die Buchstaben auch im Auslaut und vor *s* unverändert beibehalten werden.”

<sup>46</sup> Hench, p. 45ff. Streitberg is wrong when he says, # 35.8: “Hench ... beruft sich zum Beweise auf *þarf* Ph 2,25 B. — Aber die Hs. zeigt nach Braun deutlich *þarb*. Damit fällt Henchs ganze Beweisführung.” Hench is merely comparing Gothic with the other Germ. languages (cf. OI *kalfi*, OE *cealfu*; OI *suerfa*, etc.). As Moulton has pointed out (“Phonemes of Gothic”, footnote 12) final *-lb* or *-lbs* does not happen to occur in Gothic.

<sup>47</sup> See Moulton, “Stops and Spirants”, p. 6.

Since *pp* does not occur (except in proper names from the Greek), the words with *kk* (*smakka* and *sakkus*) probably are also to be seen as loans. Thus the reflexes of *p*, *t*, and *k* did not occur in Pre-Gothic as long consonants or geminates.<sup>48</sup>

#### 4.21.c *Alternation before s*

In some words, *þ* and *t* alternate with *s* before *s* (*gaqiss* to *qīþan*; *wissa*, pret. to *witan*; etc.). *s* and *z* are usually lost before *s* (cf. *drus*/*drusis*; *hatis*/*hatizis*). Similarity to other forms shows the second *s* of *gaqiss* and of *wissa* to have been at one time a *t* (cf. *andanumts* to *niman*; *mahta*, pret. to *magan*). We are thus able to distinguish three stages: (1) *þt* and *tt* become *ss*; (2) *þt* and *tt* become *st*; (3) *tt* does not change. We may affirm that Pre-Gothic did not contain geminate, or long, *s* before change (1). It was probably at this time that the second *s* in forms like *drus* was lost.

#### 4.21.d *Alternation before h*

No nasal occurs before *h*. Wherever we should expect this, we find zero (*huhrus* to *huggrjan*; *juhiza*, to *juggs*; *þulhta* to *þugkjan*, etc.). In such cases, the *u* before the *h* is not replaced by *au*. This is usually thought to indicate that there was some supra-segmental feature which kept this *u* from being replaced by *au*. This is usually thought to be length.<sup>49</sup> This is not necessary, however; let us suppose that the *u* in such forms is nasalized.<sup>50</sup> Since every non-nasalized *u* before *h* is broken to *au*, there is no need to signal this feature. Since we find *u* appearing before *h* in some other words, however (*uh*, *nuh*), it is probably best to assume that the change of *u* to *au* took place before the loss of the nasal before *h*. The appearance of the *u* before *h* in the forms cited merely indicates that they had in Pre-Gothic a nasal which was subsequently lost.

#### 4.21.e *Alternation of i, ai and u, au (before r, h, and hu)*

Almost every *i* alternates with *ai* and every *u* with *au* before *r*, *h*, and *hu*.<sup>51</sup> This may be considered as an indication that *r*, *h*, *hu* were all velar sounds. This is perhaps true, but we must add the reservation that, if this is true, they were velar sounds at the time when the change, which we must reconstruct from the above discussed alternation, occurred; they may well have changed since. We at least welcome the

<sup>48</sup> In spite of Moulton's belief that "comparative evidence suggests that Gothic had the geminates *pp*, *ff*, *bb* and perhaps *hh* ...", Pre-Gothic seems to have contained only geminate liquids and nasals and a geminate *ss* which was secondary (cf. below 4.21 c).

<sup>49</sup> Jellinek, p. 41; Braune-Helm, p. 11; Moulton is more circumspect. He says, concerning long *u*: "For /u:/ we have only such etymological evidence as Goth. *fuls* J 11,39, ON *fūll*, OE OS OHG *fūl* 'rotten'" ("Phonemes", p. 81).

<sup>50</sup> Such a proposal has been entertained, cf. Jellinek, p. 41. We might note that Icelandic scribes did not indicate nasalization in similar situations (cf. Einar Haugen, *The Oldest Norse Grammarian* [= *Language Monograph* 25], 1950).

<sup>51</sup> For a discussion and exceptions, cf. Braune-Helm, p. 13ff.; Moulton, "Phonemes", p. 80f.

information that *hu* seems to have had some articulatory feature (presumably aspiration) in common with *h*.<sup>52</sup> A. A. Hill, in an article on historical phonemics in 1936, has this to say concerning the phonemic status of the *au* from *u* before *r*, *h*, and *hu*:<sup>53</sup>

Thus the Gothic lowering of Gmc. *u*, represented in Gothic by the spelling *au*, occurs only before *r*, *h*, *hw*. There is, therefore, no evidence that it was not still a member of the *u* phoneme, since it is obviously phonetically controlled.

Since it is not mutually exclusive with *u*, I do not see how he can consider *au* as an allophone of *u*.<sup>54</sup> It is obvious that he is considering the other cases of *u* before *h*, etc. to have some supra-segmental feature. If we do not believe it was the loss of the nasal before *h* which caused the allophonic variation to become phonemicized, we must assume that the allophone of *u* before *h*, etc. fell together with allophones of another phoneme, also written *au*. Thus, if we follow Hill's line of reasoning to its logical conclusions, we must assume that the diphthong *\*au* (shown to have existed in Pre-Gothic below, # 4.21.j) had become monophthongized or that the allophone of *u* before *h*, etc. was a diphthong. All this is, of course, not necessary. The logical assumption is that the loss of the nasal in such forms as *\*þunhta* caused the phonemicization of the allophone written *au*, since *au* and *u* now both occurred before *h*. The phonemicization of the allophone *ai* of *\*i* cannot have taken place in this manner, however.

#### 4.21.f *Alternation between i and j*

*j* occurs only before vowels in Gothic; it never occurs before consonants. It also never occurs in postconsonantal position before vowels, it always forms the initial sound of the syllable in which it occurs.<sup>55</sup> *j* and *i* are shown to represent phonemes by the fact that they are in contrast in some words (cf. *iupa*, G 4,26; *juka* L 14,19).<sup>56</sup> The fact that they contrast in only a few positions, and that they regularly alternate in a paradigm (cf. Mc 9,29 *kuni*; L 7,21 *kunjis*, gen. sg.; k 11,26 *kunja*, dat. sg., etc.) shows them to have been members of the same phoneme in Pre-Gothic.<sup>57</sup>

#### 4.21.g *Alternation between u and w*

In some morphemes we find an alternation *u/w*, such that *u* appears in final position and before consonants, whereas *w* appears before vowels (*triu/triwis*). *u* and *w* are

<sup>52</sup> Especially since the loanwords and alphabet give us no clue as to the pronunciation of this character (cf. above, 2.32 h).

<sup>53</sup> *Language*, XII (1936), 19.

<sup>54</sup> Cf. Moulton, "Phonemes", p. 81.

<sup>55</sup> Cf. Braune-Helm, # 1, Anmerkung 5. On Syllabification in Gothic, see sect. 3.2.

<sup>56</sup> Moulton ("Phonemes", p. 84ff.) believed that *i* and *j* were members of the same phoneme. To do this, he had to assume that *u* represented two phonemes, and that *w* represented two phonemes (cf. above, p. 64, note 27).

<sup>57</sup> For the methodology of reconstruction of such phonemic splits, cf. Hoenigswald, "Internal Reconstruction", p. 81 ff.



in contrast in the following positions: C ( ) C — #, C ( ) #, V ( ) C — #, V ( ) #.<sup>58</sup> It will be noted that this contrast obtains only in final syllables. It is usually thought that *w* occurs in the position V ( ) # only when the preceding syllable is long, but that *u* occurs when the preceding syllable is short.<sup>59</sup> We find *lew* (R 7,8), for example, but *triu* (J 15,1). It is uncertain if length still was a phonemic feature of Gothic, however (cf. 4.21./ below). At any rate, we must assume that *w* and *u* were members of the same phoneme in Pre-Gothic.

#### 4.21.h *Alternation between hu and u*

We find *hu* replaced by *u* in the form *sium* (L 4,19) to *saihuan* (Mc 12,38).<sup>60</sup> Since this alternation occurs only in this one morpheme, we cannot be sure if it obtained throughout the language. If we can assume that it did, we welcome the information that *hu* shared some feature of articulation (presumably labialization) with *u* in Pre-Gothic.

#### 4.21.i *Alternation between ai and aj*

*ai* and *aj* alternate in the forms: *wai* (*waidedja* J 10,1)/ *waja* (*wajamerjands* t 1,13); *aiw* (M 9,33)/ *aju* (*ajukduþ* J 6,51); *bai* (L 6,39)/ *bajopum* (Mc 9,17). This shows that the *ai* in these forms at least has a diphthong as its Pre-Gothic correspondent.

#### 4.21.j *Alternation between o (u) and au (aw)*

When we observe the following sets of semantically related words: *stojan* (J 8,26), *staua* (K 11,29), *stauides* (L 7,43); *tauja* (R 7,15) *taujis* (R 13,4), *-tojis* (T 2,9), *taui* (E 2,10, nom. sg.), *toja* (E 3,7, dat. sg.), *toja* (J 8,41, acc. pl.), *tojam* (C 3,9, dat. pl.), *gatawida* (T 1,13), we see that *o*, *au* and *aw* alternate. This is usually thought to indicate that *au* was an open *ō* before vowels.<sup>61</sup> Others believe that the Pre-Gothic reflex was a long vowel plus *w*, and that the alternation arose through “Verschiebung der Silbengrenze und Verkürzung ...”<sup>62</sup> Streitberg and Jellinek believed that the *au*

<sup>58</sup> Following the practice of Moulton (cf. “Phonemes of Gothic”), I allow parentheses to enclose the “case vide”. Thus (...) is to be filled by the sounds in question, *u* and *w*.

<sup>59</sup> Cf. Braune-Helm, # 42. This view does not take the form *lasiws* (k 10,10) into consideration. Evidently this is considered to be analogy to such forms as *lasiwostai* (K 12,22).

<sup>60</sup> The only note on this alternation to be found in the literature is in Uppström, *Codex Argenteus*, 1854, p. ii of the “Praefatio”: “Quomodo, nisi lenitate soni concessa, siuns a saiwan derivari potuit?” He seems to have the same idea as that expressed here.

<sup>61</sup> Cf. H. Paul, “Gotisch *ai* and *au* vor Vocal”, *PBB*, VII (1880), 152-160; H. Paul, “Noch einmal gotisch *ai* und *au* vor Vocal”, *PBB*, VIII (1882), 210-222; Braune-Helm, p. 19; Boer, p. 50; van der Leyen, p. 55; Feist, p. 84 b. The literature is too large to cite all of it. The arguments cited are usually comparative.

<sup>62</sup> Cf. J. Schmidt, “Die vertreter von urspr. *āv*, *ōv* in den germanischen Sprachen”, *ZvS*, XXII (1882), 1-19; O. Bremer, “Germanisches *ē*”, *PBB*, XI (1885), 56; Braune-Helm, p. 20. There is a large literature on the subject.

in these forms was a diphthong.<sup>63</sup> I think that we are entitled to consider the following as sure: there must have been a sound with a labial component; this sound became *au* in some cases, *o* in others, and *aw* in still others. It is quite probable that this sound was an *\*ou* (*u* with allophones of [u] and [ʊ]) or an *\*au*. The alternation cannot be used for conjectures as to Wulfila's Gothic. Since *u* does not occur before vowels, we cannot be sure that *au* does not replace it in forms which have no by-forms in *o* (such as *gabauan*, Mc 4,2; *trauaida*, M 27,43). A rule that *o* is replaced by *au* before vowels is broken by such forms as *waiwoun* (M 7,25), *lailoun* (J 9,28).<sup>64</sup> Since *o* and *au* occur in identical environments (*tauja*, *toja*) they doubtless represent different phonemes.

#### 4.21.k *Alternation between e and ai*

In some reduplicating verbs (*saian* Mc. 4,3; *waiandin* J 6,18) and in the word *armaion* (M 6,1) we find an *ai* instead of an *aj* before vowels. Beside the form *saian*, we find the form *-seþs* (J 7,7), which indicates that *ai* and *e* were members of the same phoneme in this morpheme in Pre-Gothic. Structural parallels with other members of the reduplicating class point to this also (cf. *waiān/waiwoun*; *letan/lailotun*). The views of this phenomenon parallel those on the alternation *au/o* discussed above. Some believe that *e* becomes *ai* (an open *e*-sound) before vowels.<sup>65</sup> Some believe that the Pre-Gothic reflex of *e* and *ai* in these forms was a long vowel plus *j*, which yielded the alternation by "Verschiebung der Silbengrenze und Verkürzung".<sup>66</sup> Others insist that this *ai* also was a diphthong.<sup>67</sup> The following is clear: the Pre-Gothic reflex of *ai* and *e* in the above words was different from their usual reflex.

##### 4.21.1 *Alternation between ei and ij*

We find that *ei* and *ij* regularly alternate in the weak verbs of class I and in the ja-stem nouns and adjectives. The alternation, as it is usually described in the handbooks, has the following patterning:

VCC — eis (hairdeis)  
(e, ei, au, ai, o, u) — C — eis (*lekeis*, *sokeiþ*, *sleiþeiþ*, *brukeiþ*, *lauseiþ*, *baideis*)  
(a, i) — C — jis (*lagjiþ*, *bidjiþ*)

This indicates that *ei* and *ij* were members of the same phoneme (or cluster of phonemes) in Pre-Gothic, and that their alternation was governed by the length of the

<sup>63</sup> Streitberg, 5th and 6th ed., p. 78; Jellinek 43. This is categorically denied by Marstrandner *NTfS*, III, p. 312, and is said to be unlikely by Moulton ("Phonemes", footnote 37). Pisani, p. 119, supports Streitberg and Jellinek. The literature is quite extensive. The arguments cited are usually based on comparative evidence.

<sup>64</sup> Cf. Braune-Helm, p. 16.

<sup>65</sup> Cf. Paul, *opera cit.* (footnote 1, p. 14); Bremer, p. 56; Braune-Helm, p. 16.

<sup>66</sup> Braune-Helm, p. 16; Kluge, *Festschrift Vietor*, 106ff.; van Helten, "Zur Behandlung von *\*-ōwj*, *\*-ōwi* und antevokalischem *e* im Vorgotischen", *PBB*, XXI (1896), 467-471; Pisani, p. 119f.

<sup>67</sup> Jakobsohn, "Zwei Probleme der got. Lautgeschichte", *ZvS*, XLVII (1915), 92ff.; Schulze, "Gotica", *ZvS*, XLI (1907), p. 175; Pisani, p. 120 — all three because of the *j* which is often found in such forms as *saian* (cf. above, section 3.12.7).

preceding syllable. Since *e*, *ei*, *au*, *ai*, *o*, *u* pattern like VC, it is natural that we should consider them as clusters also, as VV, and this is what is usually done.<sup>68</sup> It must be asserted, however, that this alternation merely proves that these vowels were long in Pre-Gothic, it does not prove anything for Gothic itself. It would make it probable that these vowels were long in Gothic were it not for one consideration: We find our rule broken several times (cf. *kunþjis*, k 2,14; *gawairþjis* L 1,79; *reikjis* K 15,24; *awepþjis* K 9,7; etc., ca. 40 examples). Unless we are willing to set these aside as scribal errors (they occur in all parts of our corpus), the fact that the sets *VCC* — *jis* and *e* — *C* — *jis* are possible indicates a breakdown in the old system in which length was phonemic.

#### 4.21.m Loss of final vowels

If we must assume, as we have above (4.21.g), that *u* and *w* were members of the same phoneme in Pre-Gothic, we must assume that they occurred in mutually exclusive environments. Yet, such forms as *gaidw* and *dauþu* (both Ph 2,30) show that they do not occur in mutually exclusive environments in Gothic. Since *w* occurs only before vowels except in final syllables before zero and *-s*, and since *u* never occurs before vowels, we must assume that a vowel has been lost in such forms as *gaidw* (Ph 2,30), *lew* (R 7,8), *marisaiw* (L 8,22), *hlaiw* (M 27,66), etc. This assumption is supported by forms like *huns-la-stadis* (M 5,24) as compared to *huns-l* (M 9,13); *gilstra-meleins* (L 2,2) as compared to *gilstr-* (R 13,6). This loss of final vowels could only have come about because of a heavy stress on a preceding syllable. In such mono-syllabic words as *gaidw*, it would have to have been the first syllable; we cannot be sure about compounds. The presence of a number of pairs like *and-haffands* (M 8,8): *anda-haft* (k 1,9); *andnimiþ* (M 10,40): *anda-nemis* (Ph 4,15) indicates perhaps that nouns were accented differently from verbs.<sup>69</sup> But the presence of such forms as *unþa-þliuhand* (Th 5,3) and *und-rinnai* (L 15,12) shows that this situation, if indeed it obtained in Pre-Gothic, has been changed in Pre-Gothic, or that analogy has introduced *unþa* into a form where it does not, historically speaking, belong. At any rate, we must assume a stress accent for Pre-Gothic. This accent was, most probably, non-phonemic at first. By analogy, or other unknown causes (perhaps the formation of verbal nouns which retained the old verb-stress) this accent became phonemic, i.e. no longer predictable. Roman Jakobson has formulated a rule which states that phonemic length and phonemic stress never occur in the same language.<sup>70</sup> Although Martinet has shown that they do occur in at least one language,<sup>71</sup> we may take this as a general

<sup>68</sup> Cf. Jellinek, p. 41, who states the case in quite modern terms.

<sup>69</sup> Cf. Wrede, # 83ff.; Streitberg, # 36.2; Jellinek, p. 48ff.

<sup>70</sup> Cf. Jakobson, *O cesskom stiche* (Berlin, 1923), p. 23ff., "Die Betonung und ihre Rolle in der Wort- und Syntagmaphonologie", *TCLP*, IV, p. 234. The rule was accepted by Trubetzkoy (*TCLP*, I, p. 42), de Groot (*TCLP*, IV, 117f.) and by Heffner, "A Note on Phonological Oppositions", *Harvard Studies and Notes in Philology and Literature*, XVII (1935), 104.

<sup>71</sup> A. Martinet, "Description phonologique du parler franco-provençal d'Hauteville", *Revue de Linguistique Romane*, XV, p. 1.

rule.<sup>72</sup> If we accept this rule, we may see in the loss of length distinctions (cf. above, 4.21.1) the arising of phonemic stress. This may appear to some a rather meager argument, and so it is; but every scrap of evidence is important where evidence is so scant.

It is often maintained that the above discussed-phenomena indicate that Gothic had predictable (i.e. non-phonemic) stress.<sup>73</sup> It is, however, methodologically unsound to assume that the cause of a change is still present just because the effect is present. We are able to state with certainty most of the facts of OHG accentuation, since we find weakening of final syllables in MHG, but this does not mean that MHG has the same accentual pattern as OHG.

### 4.3 CONCLUSIONS

It is now necessary to set down our tentative conclusions in the light of all the above discussions before discussing the value of comparative evidence. As was pointed out in 2.3, there are two reasons for this: it allows us to better evaluate our criteria, and it provides a basis for the discussions to come. It is especially useful here, since the point is taken in Chapter 5 that comparative evidence has been stressed too heavily in previous studies. If we are to place comparative evidence in its proper light, we must know what the other evidence has told us. It is also a convenient summary of all our previous discussions. I have not here given a theoretical discussion of the relative weight to be accorded to each criterion, since this would be presumptuous at this time. Whenever two criteria give conflicting evidence, the weight to be accorded each is discussed in its proper place.

#### 4.31 *Pronunciation of the vowels (tentative conclusions)*

4.31.a — *a*. No further evidence has been brought to bear in Chapters 3 and 4. Our assumption is still as given in 2.31.a.

4.31.b — *ai*. The following considerations point to a monophthongal pronunciation of *ai*: (1) it is derived from the Greek digraph αι (1.11), which was pronounced [ε] (2.21); (2) it is used to render Greek αι and ε in Biblical names (2.11.2) (ε was probably pronounced [ε], 2.21); (3) if Wulfila had two phonemes (*a<sup>i</sup>* and *ái*), there were means of signaling this available to him (2.11.21, 4.12 B). Since we can cite analogous environments for (supposed) monophthongs and diphthongs, this argument is particularly good. If these words were not homophonous to Wulfila, he would not have failed to attempt to signal this; (4) it is not broken in the Gothic syllabification practice (3.3).

<sup>72</sup> Haudricourt and Juilland, *Essai pour une histoire structurale du français* (Paris, 1949), 17, 121.

<sup>73</sup> Cf. the authorities cited above, note 69.

One consideration points to a diphthongal pronunciation of *ai:ai* alternates with antevocalic *aj* (4.21.h). Points (1), (2) and (4) above are to be preferred, without considering the numerical superiority, to this point. They deal with the language as Wulfila spoke it, or as Wulfila indicated it was spoken. That a feature existed in a previous stage of a language does not show that it exists at a later stage. Things reconstructed by internal reconstruction may go back to a very early stage of the language — the reconstructed change *tt ss* (4.21.c) is considered by some to be “voreinzelsprachlich”, or at least partly so.<sup>74</sup>

Evidence thus far considered points to a monophthongal pronunciation of *ai*. We can say no more about its absolute pronunciation than was said in 2.31.b, but cf. below.

4.31.c — *e*. The only further evidence to be added to our discussion above (2.31.c) is that the scribes confused *e* and *ei* (3.13.1 a and b), and that *e* is the Gothic reflex of a Pre-Gothic long vowel (4.21.L). The first point cannot be used to determine Wulfila’s pronunciation (3.03, Conclusion to Chapter 3) unless we assume that the scribes spoke the same dialect as Wulfila. The second point is a welcome one. It is a well-known phenomenon that long vowels tend to be closer than short vowels, and the converse is also true.<sup>75</sup> If *e* was a long vowel in Pre-Gothic, this makes it probable that it was a closer vowel than *ai* (cf. 2.31.c) in Gothic. The scribal confusion also points to this.

4.31.d — *i* and *ei*. We can only add to the discussion above (2.31.d) that *ei* is the reflex of a Pre-Gothic long vowel or diphthong (4.21.L) or a vocalic nucleus (*\*ii*, cf. the alternation *ei, ij*). Since long vowels are usually closer than short vowels, *ceteris paribus*, *ei* was probably closer than *i*. This helps us out of our dilemma (2.31.d) concerning the relative position of *ei* and *i*. Since *ei* is never separated in word-breaking, it was doubtless a monophthong in the language of the scribes. *ei* occurs ca. 800 times in a list frequency, so the lack of word-breaking cannot be considered fortuitous.<sup>76</sup>

4.31.e — *u*. We can add nothing to the discussion of *u* given above (2.31.e). The fact that some *u*’s have a Pre-Gothic long vowel correspondent (cf. *hrukeis*, *brukeis*) might be thought to indicate that Gothic *u* represented both long and short *u*. Such an argument cannot be taken seriously, especially since *hruk* and *bruk* could be taken to be long syllables, just as *brikan* is a verb of the IVth class (Braune-Helm, # 33).

4.31.f — *o*. The only evidence to be added to our discussion above (2.31.f) is that the scribes confused *o* and *u* (3.13.2), and that *o* is the reflex of a Pre-Gothic long vowel

<sup>74</sup> Cf. Streitberg, *Urgermanische Grammatik* (Heidelberg, 1896), p. 115.

<sup>75</sup> Paul Passy, *Etude sur les changements phonétiques*, Doctoral Dissertation (Paris, 1890), # 295.

<sup>76</sup> Cf. Martin Joos, “Statistical Patterns”, p. 37 for the statistics.

(4.21.1). The first point cannot be used to determine Wulfila's pronunciation (3.03, Conclusion to Chapter 3) unless we assume that the scribes spoke the same dialect as Wulfila. The second point is a welcome one. It is a well-known phenomenon that long vowels tend to be closer than short vowels, and the converse is also true (4.31.c). If *o* was a long vowel in Pre-Gothic, this makes it probable that it was a closer vowel than *au* (cf. 2.31.f) in Gothic. The scribal confusion points to this also.

4.31.g — *au*. The following considerations point to a monophthongal pronunciation of Gothic *au*: (1) it is used to render *o* in Biblical names; (2) if Wulfila had two phonemes (*au* and *áu*), he would have signaled the phonemic difference if means were available, and means were available (2.11.21, 4.12.B). We can cite a pair which is almost minimal, *þlauhs* (Mc 13,8, the lack of an acc. sg. \**þlauh* is surely fortuitous), supposed to be a monophthong, and *-þlauh* (k 11,33), with a supposed diphthongal pronunciation. If these words were not homophonous to Wulfila, he would not have failed to attempt to signal this; (3) it is not broken in syllabification (3.21); (4) we cannot use the scribal confusion of *au* and *u* (3.13.2) to show that Wulfila had a monophthongal pronunciation (3.03 and Conclusion to Chapter 3).

One criterion points to a diphthongal pronunciation of *au*: *au* alternates with antevocalic *aw* (4.21.j). Points (1) and (3) outweigh this consideration (cf. above 4.31.b), since they concern Wulfila's dialect and this criterion shows only that *au* was a diphthong at an earlier stage of the language.

The evidence points to a monophthongal pronunciation of *au*. *au* was a more open sound than *o* according to the discussion in 4.31.f. (cf. also 2.31.g).

4.31.h — *iu*. The fact that *iu* is broken in syllabification indicates that it was a diphthong in the scribe's language, or perhaps in Wulfila's language (3.21).

#### 4.32 *Pronunciation of the consonants (tentative conclusions)*

4.32.a — *p, t, k*. There are no further conclusions to be added to the discussion in 2.32.a.

4.32.b — *b, d, g*. The fact that *b* and *d* alternate with *f* and *þ* finally and before *-s* indicates that they were spirants in post-vocalic position in Pre-Gothic (4.20, 4.21.a). The fact that they are not replaced by *f* and *þ* in final position after nasals (*lamb, band*) and liquids (*þarb, gild*) indicates that they were not spirants in this position in Pre-Gothic. Structural pressure seems to indicate that *g* was also a spirant in Pre-Gothic (4.21.a). The evidence of the alphabet (1.11) and Biblical names (2.11.1, 2.32.b) may point to a spirant pronunciation, but this is not certain. If we allow the Pre-Gothic structure to remain in Gothic, *b, d, g* were spirants in some positions, stops in others. They could be stops (or spirants) in all positions, however.

4.32.ba — *The velar nasal*. The fact that we sometimes find *n* written for *g* before *g*, *k*, *q* indicates that it was a nasal sound (3.11.1), doubtless a positional variant of *n*, in the language of the scribes. Since the velar nasal occurred only before *g*, *k*, *q* and *n* never occurred there, it was a part of the /n/ phoneme in Wulfilian Gothic.

4.32.bb — *ggw*. The fact that *ggw* patterns like other clusters containing a nasal or liquid points to the presence of a nasal in this cluster (4.11.2). This is supported by the fact that *gg* is used to render a velar nasal plus *g* in Biblical names (4.32.ba), and by the fact that *ggw* parallels the *ggr* in *huggrjan*.

4.32.c — *h*. Evidence of the alphabet (1.13) and scribal errors (3.13.4) points to a value as a glottal spirant. The criterion of phonemic opposition may indicate that *h* is a velar spirant (4.11.1.b), but this is not certain. Structural pressure forces us to assume that *g* had a voiceless spirant allophone in final position (4.21.a). To account for the fact that *g* is not then replaced by *h*, we must assume that *-h* had a different point of articulation from *g*.

4.32.d — *s* and *z*. We can only add that *s* and *z* were correlatives in Pre-Gothic, since *z* is replaced by *s* in final position (4.21.a). As far as pronunciation goes, our ideas have not changed (cf. 2.32.d).

4.32.e — *m*, *n*, *l*, *r*. The discussion is still as above (2.32.e).

4.32.f — *f*, *þ*. Nothing can be added to 2.32.f.

4.32.g — *q*. The concept of patterning points to a value as a cluster for *q* (4.11.1). The fact that it is treated as a single consonant in syllabification (4.14.2) cannot be used as an argument to the contrary; Wulfila had no way to break up his sign *α* since he used *k/w* to indicate some kind of juncture. Nothing can be added concerning pronunciation (cf. 2.32.g).

4.32.h — *hu*. The fact that *i* is broken to *ai* before *hu* indicates that it had a *h*-sound (*rw* occurs, cf. *arwjo* J 15,25), as does also the fact that it patterns like *h* to some extent (no nasal occurs before *hu* just as nasal does not occur before *h*) (4.21.e). This is, of course, only true for Pre-Gothic. The fact that *hu* is replaced by *u* before *n* (4.21.h) indicates that *hu* had a labial component in Pre-Gothic. This is our only criterion for the pronunciation of *hu*. The remarks made concerning whether *q* was a close-knit sequence or a cluster apply to *hu* also (cf. above 4.32.g).

4.32.i — *w*. Since *w* alternates with *u* (4.21.g), we must assume that it had a labial articulation as one of its features, that it was a non-syllabic *u*, in Pre-Gothic. *w* and *u* were members of the same phoneme in Pre-Gothic. The alphabet (1.11) and the

criterion of Biblical names (2.11, 2.32.i) also point to a labial articulation. We cannot determine whether the tongue mid was raised during the production of the sound.

4.32.j — *j*. Since *j* alternates with *i*, we assume that it was a consonantal allophone of a phoneme \**i* in Pre-Gothic (4.21.f). The evidence of Biblical names points to a value as a consonantal *i* in Gothic (2.11, 2.32.j).

#### 4.33 *General Conclusions*

The above discussion shows that the criteria of the alphabet and Biblical names are the most useful in determining the articulatory features signaled by Wulfila's signs. The scribal errors tell us little about Wulfila's pronunciation. Morphophonemic variations can give us useful hints, since they point to the existence of certain articulatory features in Pre-Gothic. Evidence obtained from the configuration of the system cannot inform us concerning the phonetic values, but only concerning the phonemic status of the sound signaled by a particular sign (whether it is a monophthong, a diphthong, a cluster, etc.). When used together, all these criteria give us some degree of certainty concerning the *relative* position of the vowels and, perhaps, the consonants. We know the relative position of the vowels in tongue height, and the relative position of each consonant series in point of articulation (4.11.1). We cannot be sure of the front-back relationship of the vowels, nor do we know the relative position of the consonants, or their relationship, in the orders. The above conclusions (4.32) set down, I believe, all that can be said concerning the phonetic values of Wulfila's signs without using comparative evidence.



## COMPARATIVE EVIDENCE

5.0 In the previous chapters only evidence to be garnered from an examination of the manuscripts themselves was considered; we have dealt with no evidence which did not immediately concern the language found in the manuscripts. In the present chapter we will consider the comparative evidence, evidence afforded by a comparison with the other Germanic and Indo-European languages. There are two aspects to this study: (1) what does comparative evidence add to our knowledge of Wulfila's pronunciation? (2) What value does our discussion of Wulfila's pronunciation have for our comparative study?

5.1 THE VALUE OF COMPARATIVE EVIDENCE IN DETERMINING  
WULFILA'S PRONUNCIATION5.10 *Methodological remarks*

Before considering the value of the comparative evidence for each individual sound, it is good to be clear about the techniques involved in using comparative evidence and the premises necessary for its use.

It may be said that there are two types of comparison: (1) generic comparison, (2) genetic comparison. When we point out the resemblance between the Polynesian articles and the English articles, we are performing the first type of comparison, we merely point out a resemblance of structure.<sup>1</sup> When we say that Gothic *t* corresponds with Latin *d* (*taihun*, *decem*), we are performing the second type of comparison, since our only explanation for this correspondence is to assume a time when both these words existed in only one form. Things are genetically related only when they derive from a common source. There is only one way to set up sound correspondences between two or more languages. This is by means of cognate words. We assume, for each set of cognates we discover, that there was a time of complete unity, a time when there was only one word, existing under one form. We have no way of telling, however, whether a given reconstructed form existed at the same time as another

<sup>1</sup> On the Polynesian articles, cf. W. K. Matthews, "The Polynesian Articles", *Lingua*, II (1949), 14-31.

reconstructed form. We must assume this for practical purposes. If we find a number of concordances in grammatical form, vocabulary, etc., we say that the two languages are related.<sup>2</sup> We then set up sound correspondences using the cognates already discovered.

In doing any comparison on genetic lines we must reconstruct, either tacitly or explicitly, a stage of language in which the things compared were one. We cannot compare OHG *wēr* and Gothic *wair* without setting up a Proto-Germanic form from which both are derived. This may be expressed graphically in the following manner: Let there be a language A; let there further be two languages B and C such that B and C are derived from A:



If we move along the segment B-C, we cannot affirm anything concerning the correspondence of B to C deriving from the fact of their relationship. To compare along genetic lines, we must go along the segments B-A, A-C, or vice versa. C and B are not directly related with each other, but their direct relation is with A. We are justified in ascribing to A a feature which obtains in B and C which cannot be explained as a subsequent development. We are perhaps justified in ascribing to A a feature which is found in B, but not in C, which is not explainable as an innovation. But we are not justified in assuming that C must exhibit a feature because A (and B) have it. If C must exhibit all the features of either A or B, it cannot be different from A or B. Translated into concrete terms, we are not justified in attributing to Gothic a feature, just because this feature exists in OHG or ONorse. If we have no other evidence, we may assume that a feature which existed in Proto-Germanic is retained in Gothic. If we have other evidence to the contrary, this evidence must be given greater weight than etymological evidence. This must be true, for if we allow etymological evidence to prevail in every case, there can be no comparison. If a supposed pronunciation would entail the supposition of a sound change which is most unlikely to have occurred, we have to accord to etymological evidence a greater weight.

A further consideration should perhaps be mentioned here. We assume a time at which all the Germanic languages were one language. For practical purposes, we assume that this time coincides with the existence of Proto-Germanic. We assume that Germanic was a unified language at the time when it "broke off" from IE. This schematic way of thinking has its practical values, but we must not ignore the fact that it is only a way of thinking, and that it cannot fit the facts as they must have existed. There were speakers of Proto-Germanic, and, if these speakers lived in widely separated settlement areas, dialectal cleavage must have taken place. Pre-historic archeologists tell us that the Germanic peoples were situated in Scandinavia, Denmark, and the northern coast of Germany during the third millenium B.C., and

<sup>2</sup> For the methodology of discovering cognates and reconstruction, cf. Meillet, *Introduction*, Chapter I, and Bloomfield, *Language*, Chapter 18.

that they have not been invaded since.<sup>3</sup> If we date the time of IE unity, with Meillet, at 3000 B.C., it becomes likely that there was not a time of complete unity in Proto-Germanic, except as a dialect of IE. At any rate, we must reckon with some date anterior to ca. 2000 B.C. for the time of unity.<sup>4</sup> Many changes could have come in between 2000 B.C. and 400 A.D.

The foregoing discussion brings us to another point which has not been often enough insisted on in the past.<sup>5</sup> The date of attestation is not an index to the archaic character of a language. Lithuanian, attested first in the 16th century, is often thought to be as archaic as Greek or Latin, attested ca. 700 B.C. Latvian is most certainly not as archaic, i.e. does not preserve as many old features, as Lithuanian, yet both are attested at ca. the same time.<sup>6</sup> We cannot insist, therefore, that Gothic must be more archaic than the other Germanic languages in every feature.

### 5.10.1 *Previous studies and critique*

In previous studies, the criterion of etymology has tacitly been accorded the greatest weight. All other things being equal, an etymological argument has been allowed to prevail over any or all other criteria, in spite of the fact that almost all authorities say that evidence of alphabet derivation and transcription of Biblical names is to be preferred.<sup>7</sup> Even those who deny the use of etymology in some cases believe it has value in others.<sup>8</sup> Those who believe most in etymology also deny its value in some cases.<sup>9</sup> This much is certain: if we allow the criterion of etymology to have the greatest weight in one case, it must have the greatest weight in all cases, *ceteris paribus*.

<sup>3</sup> This is the result of the investigations of Scandinavian archeologists reported by O. Montelius, *Minnen från vår forntid*, Vol. I (Stockholm, 1917). Marija Gimbutas, in a recent book and in an article ("On the Origin of North Indo-European", *American Anthropologist* LIV [1952], 602-611) comes to similar conclusions, though her dating is somewhat later than that given here. She says (p. 609): "The diagram of North 'Indo-European' differentiation should be closed with the first half of the second millenium B.C. In the first millenium B.C., the separate groups assumed to be Germanic, Slavic and Baltic, were already differentiated into smaller cultural units." She gives no reason for her dating, but it must be from a consideration of the linguistic facts, since Karsten states (*Les anciens germains*, p. 26): "D'après les résultats de l'archéologie préhistorique ..., le nord de l'Allemagne et les pays scandinaves étaient déjà occupés par les germains dès le commencement du néolithique, c'est-à-dire au plus tard au cours du troisième millénaire avant J.-C." He bases himself on the fact that the burial customs have not changed since the end of the glacial period.

<sup>4</sup> It should perhaps be noted that the technique of "glottochronology" shows that Germanic, Slavic, and "Romanic" must have been one language ca. 1500 B.C., cf. Swadesh, "Archaeological and Linguistic Chronology of Indo-European Groups", *American Anthropologist*, LV (1953), 351. Until refinements have been made in this technique, it will be useless to the linguist. In contradistinction to the results cited by Swadesh, Lees ("The Basis of Glottochronology", *Language* XXIX [1953], 126) has Germanic and Romance (exemplified by French and English) separating at some time between 1509 and 491 B.C. This discrepancy, and the large margins of error necessary, and the possibility of errors in the corpus investigated, render this method useless.

<sup>5</sup> Cf. *supra*, p. 54, footnote 94.

<sup>6</sup> Cf. Meillet, *Introduction*, p. 73.

<sup>7</sup> Cf. Mossé, *Manuel*, p. 37; van der Leyen, p. 16; van Hamel, p. 25; Jellinek, p. 30; Streitberg, p. 30.

<sup>8</sup> Cf. below, 5.11.2.bb.

<sup>9</sup> Cf. below, 5.11.1.d.

If we relegate it to a position of lesser importance in one case, we should relegate it to this position in all similar cases. If *ai* is assumed to have also a diphthongal pronunciation because of etymological considerations, why should not *ei* be assumed to have also a diphthongal value?<sup>10</sup> It may be objected that no Germanic language retains a diphthongal pronunciation of IE *\*ei*. But why should we not consider *i* to signal two phonemes, as Wrede has done, since the etyma in the other Germanic languages show two phonemes?<sup>11</sup> Or why not consider *e* to signal two phonemes, since it corresponds to two phonemes in West Germanic?<sup>12</sup>

On the other hand, if we deny that etymology can tell us that *ai* and *au* had two values each, how can we affirm that it can tell us that *ggw* had two values? The case is exactly analogous to the case of *ai*, *au*.<sup>13</sup> In previous studies, there is no criterion, either implicit or explicit, which can be used to determine when etymological considerations are to be given greater weight than other considerations, and when they are to be given lesser weight.

One other point should be mentioned before a discussion of the value of etymology in individual cases is taken up. As E. Hermann has pointed out, all reconstruction of the sounds of a Proto-Language must be based on the sounds of the attested languages.<sup>14</sup> If we use the (assumed) sounds of the proto-language to determine the sounds of the attested language or languages, we are guilty of arguing in a circle, since that which is postulated is proven by the fact of its postulation. I would not advocate dropping the practice of performing such circular arguments, since, as Brugmann has said, they are one of the real *raisons d'être* of comparative linguistics.<sup>15</sup> It must be realized, however, that the results of circular arguments can never be certain without support from sources outside the circle.

### 5.11 Discussion of each individual sign

In this section the value of etymological considerations is discussed for each sign in turn.

#### 5.11.1 Vowels

5.11.1.a — *a*. A comparison of Gothic *dags* with OHG *tag*, OE *daeg*, OIcel, *dagr*, OS

<sup>10</sup> Cf. below, 5.11.1.c.

<sup>11</sup> Cf. below, 5.11.1.d.

<sup>12</sup> Cf. below, 5.11.1.f.

<sup>13</sup> This similarity was noted by the first person to propose two values for *ggw*. In a review of O. Schade's *Paradigmen zur deutschen Grammatik* in the *Zeitschrift für die österreichischen Gymnasien*, XIX (1868), 855, W. Scherer proposed this idea, adding: "Eine doppelte Geltung des *ggv* anzunehmen, wird sich niemand bedenken, der mit Grimm dem *ai* und *au* eine doppelte Geltung zuschreibt, die nur wie in unserem Falle durch Herbeiziehung der übrigen germanischen Sprachen für jedes einzelne Wort zu stellen ist." The problem of *ggw* has been a thorn in the side of those who believe in a single value for *ai* and *au* since Scherer's time, cf. below 5.11.2.bb.

<sup>14</sup> E. Hermann, p. 33f.

<sup>15</sup> Brugmann, I<sup>3</sup>, p. 1: "Jede Einzelsprache hat zur Reconstruction des uridg. Sprachzustandes mitzuhelfen, und jede erhält wiederum von dort her durch das, was die andern Sprachen zur Erhellung des gemeinsamen Ausgangspunktes beitragen, Aufklärung für sich." Cf. also p. 30f.

*dag*, shows us that we may reconstruct the Proto-Germanic reflex of Gothic *a* as a vowel which was relatively low and relatively central. Thus the sound value arrived at by using etymology and that arrived at in the previous discussions (4.31.a and 2.31.a) are in agreement.

5.11.1.b — *ai* and *au*. It is well to consider *ai* and *au* together, since they are parallel in all aspects. It is usually affirmed, on etymological grounds, that *ai* and *au* represent both monophthongs and diphthongs in Gothic,<sup>16</sup> since the etyma in the other Germanic languages show both diphthongs and monophthongs (Goth. *bairiþ*, OHG *birit*, OS *birid*; Goth. *ains*, OHG *ein*, OIcel. *einn*; Goth. *sauhts*, OIcel. *sott*, OHG *suht*; Goth. *augo*, OIcel. *auga*, OHG *ouga*). Consideration of internal evidence has already led us to consider this possibility, and it has been rejected (4.31.b, 4.31.g). Since the reasons for rejecting the evidence of morphophonemic variation are the same as those advanced for rejecting etymological arguments in this case, they need not be repeated here. If we reject the witness of internal reconstruction, we must certainly reject the witness of etymology in this case. The fact that ON and OHG still have diphthongs in corresponding words tells us only that Proto-Germanic had diphthongs in these words. The course of monophthongization in Romance (Proto-Romance *\*ai* and *\*au*) shows us that various sister languages may exhibit monophthongization at various dates.<sup>17</sup> The fact that diphthongs in OHG (ca. 800) correspond to *ai* and *au* in Gothic (ca. 400) cannot be construed to mean that Gothic *ai* and *au* must be interpreted as diphthongs in the corresponding etyma. Since the evidence of the other criteria is so clear in this case, we must give them more weight than etymological considerations.

5.11.1.c — *ei*. Jakob Grimm proposed a double value for *ei*, parallel to the double value he proposed for *ai* and *au*, on etymological grounds.<sup>18</sup> This theory of a double value for *ei* has been given up by later scholars on the grounds that no Germanic language exhibits *ei* in the corresponding words, (Goth. *swein*, OHG, OS *swīn*, OIcel. *swīn*; cf. Lat. *suīnus*; Goth. *gateihan*, OHG *zīhan*, OS *tīhan*; cf. Osc. *deikum* and Skt. *deksyati*).<sup>19</sup> This argument is only valid if it be construed as proof that

<sup>16</sup> It is usually affirmed that *ai* and *au* indicated both monophthong and diphthong. *Monophthong* and *diphthong* are phonetic, not phonemic, terms. It is not really a question of monophthong vs. diphthong, but a question of whether *ai* and *au* represent two phonemes or one phoneme each. The use of the term *monophthong* vs. the term *diphthong* should not be construed as implying a close phonetic identification; they are used here because they have been used so often in previous treatments. No one can say that *ai* did not represent a diphthong of the type [ej]. In the older literature, one feels, the question was seen as one of phonetic identification.

<sup>17</sup> Haudricourt and Juilland, p. 17ff.

<sup>18</sup> Cf. Braune-Helm, # 16, Anm. 3.

<sup>19</sup> This was first stated by Bopp (cf. Weingärtner, p. 35). Unfortunately neither Streitberg, Jellinek, nor Braune-Helm bother to give reasons for not assuming a double value for *ei*, analogous to their double value for *ai* and *au*. Indeed, Streitberg's reasoning, if applied to *ai* and *au*, would lead to an assumption of a single value for these digraphs also: "Dass got, *ei* einen *langen Vokal*, keinen Diphthong bezeichnet, ist aus der monophthongischen Geltung des griech. *ei* zu folgern. Dieser Schluss wird durch die Tatsache bestätigt, dass *ei* auch dort gebraucht wird, wo ein durch Ersatzdehnung gelängtes *i* vorliegt (p. 55)."

Proto-Germanic had no *ei* in the corresponding etyma.<sup>20</sup> Since *ei* is attested in one runic inscription (*teiwa* on Helmet B of Negau), and in two transcriptions into other languages (*Alateiviae* on the Xanten inscription and Karelian *runko-teivas*),<sup>21</sup> it seems that we must reckon with a Proto-Germanic \**ei*.<sup>22</sup> Unless we assume that these examples are spurious, or that they all stem from a time when the Germanic languages were still one language,<sup>23</sup> we cannot affirm that the absence of an *ei* in the other Germanic languages proves the absence of an *ei* in Gothic. If we affirm that *ai* and *au* represent two phonemes each on etymological grounds, we must affirm that *ei* signals two phonemes on the same grounds. This most scholars would not be willing to do.

I consider *ei* to signal one phoneme on the basis of alphabet and loanword evidence, giving less weight to the consideration of etymology, as indicated above (5.10).

5.11.1.d — *i*. Almost all scholars consider *i* to signal one phoneme, a short, or open, *i*-sound.<sup>24</sup> A comparison of Gothic *stilan* with OS, OHG, OE *stelan*, OIcel. *stela* and Gothic *fisks* with OS, OE *fisc*, OHG *fisk*, OIcel. *fiskr* shows us that we must reconstruct two "sounds" as the Proto-Germanic reflex of Gothic *i*, however. Basing his argument on the fact that the Ostrogothic reflex of Gothic *i* is both an open *i*-sound and an open *e*-sound, Wrede proposed to consider the sign *i* as representing two phonemes in Gothic.<sup>25</sup> His Ostrogothic evidence has, however, been considered

<sup>20</sup> Cf. above, 5.10.

<sup>21</sup> Cf. Konstantin Reichardt, "The Inscription on Helmet B of Negau", *Language*, XXIX (1953), p. 307; Hirt I, p. 38f.

<sup>22</sup> Cf. *ibid.*, p. 307. Hirt denies that *Alateiviae* is of any importance, since "*ei* die übliche lat. Schreibung für *i* war (!)". He says, concerning *runko-teivas*: "Bedeutsamer ist karelisch *Runko-teivas* (Karsten, *Lehnwortstud.*, 5). Doch wissen wir nicht, wann dies entlehnt ist (p. 39)." It is unfortunate that Reichardt did not see fit to give his reasons for rejecting Hirt's view. As we see by the fact that Hirt cites Karsten, he assumes that there is a possibility of pre-Germanic loans into Finnish and Lappish. Karsten's stand on this matter is well-known. His idea that there were pre-Germanic loans is based on a few loanwords, such as *rengas* (cf. OHG *rinc*), *kansa*, etc. (cf. Karsten, *Lehnwortstudien*, p. 200), and on the supposed date of the Germanic consonant shift. Karsten himself gave up this idea in his last book on the subject ("Finnar och Germaner", *Meddelanden från Föreningen för nordisk Filologi i Helsingfors*, Argang X, p. 628). It was expressly rejected by a majority of the scholars in the field, cf.: B. Collinder, "Die urg. Lehnwörter im Finnischen", *Skr. utg. av kungl. Hum. Vedenskapssamfundet i Uppsala*, Vol. 28 (1934), and "Supplement und Wortindex", Vol. 34 (1943), especially the authorities cited p. 3ff.; J. Fourquet, *Les mutations consonantiques* (Strassburg, 1948), p. 64f.

<sup>23</sup> The dating of the inscription is uncertain, and some authorities believe that the Germanic languages were essentially one until the beginning of the Christian era (cf. Prokosch, p. 27). Such an assumption is quite daring in view of the fact that the Germanic tribes were so widely distributed geographically, especially during the bronze age (cf. above, footnotes 3 and 4).

<sup>24</sup> Braune-Helm, # 9; Jellinek, p. 45f., assume that *i* signaled a short vowel, Braune-Helm without indicating its quality. Jellinek states that it was, qualitatively speaking, the same as *ei*. Wrede, # 13, and Streitberg, p. 54f., state specifically that *i* was an open *i*-sound. Most other authorities say that *i* was both an open *i*-sound and a short vowel, without attempting to state which of these features was phonologically relevant.

<sup>25</sup> Wrede, # 13: "Da dieser Unterschied auch noch im Ostgotischen des 6. Jahrhunderts erkennbar ist, wird er auch für Wulfila im 4. Jahrhundert noch bestanden haben, und sein Schriftzeichen *i* vertritt wiederum zwei, qualitativ geschiedene Laute."

suspect by competent authorities.<sup>26</sup> Even if one accepts his conclusions concerning Ostrogothic, the fact that Ostrogothic has two phonemes in the 6th century does not mean that Wulfila's Gothic must have two corresponding phonemes in the 4th. It merely indicates that there must have been two phonemes in Proto-Gothic or Proto-Germanic, and perhaps it does not even indicate this (cf. immediately below).

If we consider that etymological criteria can tell us that *ai* and *au* have two values each, in the face of other evidence to the contrary, we must give etymological criteria the greatest weight in this case also.<sup>27</sup> In favor of the usual view, it may be pointed out that Proto-Germanic *\*e* and *\*i* formed a correlative pair, and that they are mutually exclusive in a great part of their distribution.<sup>28</sup> It is, therefore, not surprising that an opposition which bore such a small functional load should cease to exist. The same argument could be used to show the relative ease with which the monophthongization of *\*ai* and *\*au* might take place, since this opposition also bore a relatively small functional load.<sup>29</sup> Those who propose a double value for *ai* and *au* must also accept a double value for Gothic *i*.

I assume that Gothic *i* signaled one phoneme, since there is nothing in the internal evidence to the contrary. Since Wulfila is known to have used *ai* in the value of an (open) *e*-sound, he would doubtless have used *ai* to indicate the open *e*-sound postulated for Gothic *i* by Wrede, unless a three-way phonemic contrast (*I/ε/ē*) existed, which is unlikely. If such a contrast had existed, Wulfila could have used *i* for */I/*, *ε* for */ε/*, *η* for */ē/*, and *ai* for */ē/*. This would have called for considerable ingenuity on his part, and cannot be used as an argument against Wrede's hypothesis.

5.11.1.e — *u*. It is usually considered that *u* merely indicates [u] or [u:].<sup>30</sup> Length will be discussed later (5.11.3). Leaving aside the consideration of length at the present, I know of no scholar who believes that *u* indicates more than one sound qualitatively speaking, though Wrede holds out the possibility.<sup>31</sup> The etyma in the other Germanic language (Goth. *gub*, OE, OS *god*, OHG *got*; Goth. *budum*, OE *budon*, OS *budun*, OHG *butum*, OIcel. *budom*) indicate that we must reconstruct two "sounds" as Proto-Germanic reflexes of Gothic *u*. Since *\*u* and *\*o* were members of the same phoneme in Proto-Germanic, it is easy to see how they could have remained,

<sup>26</sup> Cf. above, 3.01.

<sup>27</sup> Cf. above, 5.10.

<sup>28</sup> Cf. Twaddell, "The Pre-Historic Germanic Short Syllabics", *Language*, XXIV (1948), p. 139, and J. Kurylowicz, "The Germanic Vowel System", *Bulletin of the Polish Academy*, 1952, p. 52f. Both these scholars consider *\*i* and *\*e* to be in contrast in some positions, whereas some scholars would consider them to be members of the same phoneme (cf. H. Penzl, "Umlaut and Secondary Umlaut in OHG", *Language*, XXV [1949], 224, footnote 5, and the authorities cited there).

<sup>29</sup> Cf. M. Joos, unpublished dissertation, University of Wisconsin, 1940. On the concept of functional load and sound change, cf. A. Martinet, "Function, Structure, and Sound Change", *Word*, VIII (1952); Trubetzkoy, *Principes*, p. 276-290.

<sup>30</sup> Braune-Helm, p. 10; Streitberg, p. 55; Jellinek, p. 41.

<sup>31</sup> Wrede, # 18, *Anm.*

through a redistribution of allophones, members of the same phoneme in Gothic.<sup>32</sup> The usual methodology, if applied here, would cause us to interpret Gothic *u* as two phonemes, however, since all the other Germanic languages exhibit the phenomenon of phonemicization of the two former allophones into phonemes.

I assume that *u* indicated a single phoneme in Gothic, since evidence other than that of etymology points to this (4.31.e, 2.31.e), and since etymology may be construed to be not in conflict with this.

5.11.1.f — *e*. Most scholars consider that *e* indicated only one phoneme.<sup>33</sup> Some hold out the possibility that it might indicate two phonemes.<sup>34</sup> Since the other Germanic languages have two sound values corresponding to the one Gothic sign (Goth. *gadeþs*, OE *dæd*, OS *dād*, OHG *tāt*; Goth. *hēr*, OE, OS OIcel. *hēr*, OHG *hiar*), the usual methodology would seem to call for interpreting *e* as representing two phonemes.<sup>35</sup> No reasoning has been proposed for not considering *e* to represent two phonemes in Gothic.

I consider *e* to represent only one phoneme in Gothic, since there is no evidence, other than etymology, to the contrary.

5.11.1.g — *o*. A comparison of Gothic *flōdus* with OE, OS *flōd*, OIcel. *flōð*, OHG *fluot*, shows us that we may reconstruct the Proto-Germanic reflex of Gothic *o* as a relatively close mid-back vowel. Thus etymological considerations agree with the previous discussion (4.31.f; 2.31.f).

5.11.1.h — *iu*. Etymology points to a value as two vowels in sequence (Gothic *niujis*, OS, OHG *niuwī*), a value which is also indicated by internal evidence (4.31.h).

## 5.11.2 Consonants

5.11.2.a — *p, t, k*. Comparative evidence supports our previous conclusions (4.23.a and 2.32.a) (Gothic *paida*, OE *pād*, OS *pēd*; Goth. *tuggo*, OIcel., OS *tungo*, OE *tunge*; Goth. *juk*, OIcel. *ok*, OE *geoc*).

5.11.2.b — *b, d, g*. Etymology offers no new evidence, except to point to a spirantal

<sup>32</sup> Upon the loss of the final vowel in such forms as *\*gōlpa-* and *\*kusti-* (/gulpa-/ and /kusti-/), the original allophonic variation could remain, and thus a new phoneme be created; or the original allophonic variation could be given up. The first case has occurred in West Germanic, thus OHG *golt* and *kust*, whereas the second occurred in Gothic, thus *gulþ* and *gakusts*. For a discussion of the process of phonemicization, see R. Jakobson, "Prinzipien der historischen Phonologie", *TCLP*, IV (1931), 247-267.

<sup>33</sup> Cf. Jellinek, p. 41; Streitberg, p. 55f.; Boer, p. 26.

<sup>34</sup> Braune-Helm, p. 7: "Eine Verschiedenheit in der Aussprache ist für das Gotische wohl denkbar aber nicht mehr nachzuweisen."

<sup>35</sup> Wrede, # 10: "Da jedoch Wulfila auch sonst zwei verschiedene Lautwerte unter einem Schriftzeichen zusammenfasst, ist sehr wol möglich, dass die beiden etymologisch heterogenen germ. *ē* auch in der got. Aussprache unterschieden waren." There can be no doubt that Wrede's approach is more logical than that of the other Gothic scholars of his day. He is at least consistent within his theory.



pronunciation between vowels, hinted at in the internal evidence (4.32.b and 2.32.b). If Proto-Germanic had a spirant pronunciation of *\*b*, *\*d*, *\*g* in all positions at one time, we must reckon with the possibility of a spirant pronunciation of their Gothic reflexes in all positions. The fact that their reflexes in no other Germanic language are spirants in initial position cannot be taken as proof that the Gothic reflexes cannot be spirants in initial position, unless we assume that *\*b*, *\*d*, *\*g* had become stops in initial position in the period of common development. What happened in the other Germanic languages after close intercommunication with Gothic ceased can have no influence on developments in Gothic.<sup>36</sup> We cannot tell whether the basic characteristic of these phonemes was a stop or a spirant articulation.

5.11.2.ba — *Velar nasal*. Etymology agrees with the other evidence in the case of the velar nasal (cf. 4.32.ba and 2.32.ba).

5.11.2.bb — *ggw*. The first person to propose a double value for Gothic *ggw* was Wilhelm Scherer, in 1868.<sup>37</sup> The theory found rapid acceptance, and Leo Meyer, in 1869, was the last scholar to read the *ggw* in *bliggwan*, etc. as a velar nasal plus *g* plus *w*.<sup>38</sup> All scholars since Meyer have accepted Scherer's theory that *ggw* signals /ngw/ in *siggwan*, *aggwus*; but /ggw/ in *bliggwan*, *triggws*, *glaggwo*, *skuggwa*; scholars are uncertain as to whether *unmanariggwai* (t 3,3 B, -igwai A) has /ggw/ or /ngw/.<sup>39</sup> Braune believed that the writing of *ggw* did not necessarily signal /ggw/ in these words, but that the doubling of the *g* might merely indicate that the *g* in *\*/gw/* was a stop, as compared to the spirant *g* in *bidagwa* (J 9,8).<sup>40</sup> He assumed this for the parallel *ddj* and for ON *ggu* and *ggj* also. The assumption of two values for *ggw* is based on the fact that the etyma in the other Germanic languages show two values for the reflex of Gothic *ggw*. The interpretation of *ggw* as /ggw/ or /gw/ rests for the most part on the evidence of ON. It is commonly assumed that the development of Germanic *\*u̯* to *ggw* took place at a time when there was a Goto-Norse unity.<sup>41</sup> The fact that a Primitive Norse inscription (*Nhwila* on the *Naeshjaerg bracteate* of the 5th cent.) and a Finnish loan (*kuva* 'picture', cf. Goth. *skuggwa*) do not show this development indicates, however, that it cannot have arisen during a period of Goto-Norse unity, but that it is a separate development in Gothic and in Norse.<sup>42</sup> The fact that *\*ij* developed into *ddj* in Gothic, but into *ggj* in ON, gives further support for this point. Thus *ggw* and *ggv* arose independently in Gothic and ON, unless we deny the validity of this evidence.

<sup>36</sup> Cf. above, 5.10.1.

<sup>37</sup> Cf. above, footnote 13.

<sup>38</sup> Cf. Hirt I, p. 40.

<sup>39</sup> Braune-Helm, p. 40. A comparison with ONorw. *ryggja* 'to shudder' and *rogg* 'fear' (not previously suggested to my knowledge) suggests /ggw/.

<sup>40</sup> Braune, "Gotisches *ddj* und an. *ggj*", *PBB*, IX (1884), 546, footnote.

<sup>41</sup> This is the tacit assumption of all those who believe that the *Verschärfung* demonstrates that Gothic is closer to North Germanic than to West Germanic, cf. especially Hirt, I, p. 22.

<sup>42</sup> Cf. Brugmann, I<sup>2</sup>, p. 16; Noreen, *Altisländisch*, 165f. The change of [u] to [g] is not so unusual as some Gothic scholars seem to think.

The problem of the interpretation of *ggw* is extremely important in our discussion of Wulfila's writing system. If we assume that *ggw* signals two different phoneme clusters on etymological grounds, we must allow the criterion of etymology greater weight than internal evidence in other cases also. The first scholar to propose a double value for *ggw* recognized the parallelism of this case with the case of *ai* and *au*.<sup>43</sup> Later scholars have seen this parallelism also, and it has long been a thorn in the side of those who wished to assign one value to *ai* and *au*, respectively. Whenever anyone points out that Wulfila would naturally have signaled a phonemic difference if he had the means, the opposition always says, with Jellinek, "Wie stellt sich (der Forscher) zu *ggw*? Sollen wir *siggwan* und *bliggwan* reimen lassen?"<sup>44</sup> No real answer has been given to this question by the exponents of the monovalence of *ai* and *au*. They have brushed it aside with a remark that the writing of one sign for two phonemes a few times is different from writing one sign for two phonemes which occur hundreds of times.<sup>45</sup> This argument has a certain validity. It is not a matter of how many times the phones in question occur, however; it is a question of how many times they occur in contrasting environments. The number of minimal pairs in which a supposed *ai* and *au* contrast with a supposed *ái* and *áu* is necessarily small.<sup>46</sup> When we find *triggwos* (with a supposed /*ggw*/ or /*gw*/) and *siggwada* (with a supposed /*ngw*/) separated by only eleven words (k 3,14 and k 3,15), it would be indeed strange to assume that Wulfila might not have been aware of the shortcomings of his system in respect to the (supposed) ambivalence of *ggw*. If Wulfila was aware of this difference, he would have signaled it if a means lay easily at hand.<sup>47</sup> Since we assume that Wulfila was acquainted with the Latin alphabet, such a means (i.e. *ngw* vs. *ggw*) lay readily at hand. In fact, we find *n* often written for the velar nasal in our mss.<sup>48</sup>

Since the problem is of importance to the interpretation of Wulfila's system of writing, and since I may seem, to some, to be departing too radically from the scholarly tradition, it may be well to repeat the evidence for and against the single value for *ggw*. Evidence pointing to a single value (as velar nasal plus *gw*):

1. The cluster *gg* is used to signal the velar nasal plus *g* in all positions except the one in consideration (before *w*) (2.32.bb).
2. The fact that *ggw* is always divided in the same manner in our manuscripts points to a single value in the language of the scribes (3.23).
3. The fact that *ggw* patterns like other clusters containing a nasal (4.32.bb), and like the cluster *ggr*, points to a cluster of nasal + *gw*.

<sup>43</sup> Cf. above, footnote 13.

<sup>44</sup> Jellinek, "Got. *ai* und *au*", *ZfdA*, XLVI (1929), 118.

<sup>45</sup> Cf. Hirt I, p. 40: "Aber das sind wirklich nur ein paar Fälle, während *ai* und *au* unzählige Male vorkommen."

<sup>46</sup> Cf. above, 4.13, and Heffner.

<sup>47</sup> Cf. above, 4.13.

<sup>48</sup> Cf. above, 3.11.1. The fact that the first *g* in *bliggwan* is never written *n* might be cited as indication that the *ggw* here was different from the *ggw* in *siggwan*, for example. The first *g* in *siggwan* is also never written *n*.

4. The verb *bliggwan* patterns like the verbs of the third class (nasal plus consonant) rather than those of the second, since it has a preterite singular *-blaggw* (J 19,1), instead of the *\*blau* we should have expected (cf. OIcel. *hoggua/hiu*).<sup>49</sup> It could be assumed, of course, that the *ggw* of the preterite singular is merely an analogy to the other forms.
5. If a difference existed, Wulfila possessed a means of signaling it. He could have abandoned his *gg* for an *ng* (since he knew Latin).

The only real evidence we can cite against assuming a single value for *ggw* is comparative. There may be said to be some evidence in the writing system itself. The graphic sign *g* signals two pronunciation types: /g/ and /n/. What is then more natural than that Wulfila should have used his *g* to signal both pronunciation types before *g*? This cannot be said to be evidence of the writing system, however, for we know from the alphabet evidence and the evidence of Biblical names that Wulfila used the digraph *gg* to signal /n/ plus /g/. The possibility that he may have used *gg* to mean two different things is made improbable by the fact that we know he had ready at hand a means of signaling a difference between /gg/ and /ng/. In fact, the fact that the velar nasal was actually a part of the /n/ phoneme should have suggested the use of *n* to signal the velar nasal. That he used *g* to signal a sub-phonemic variant under the influence of the Greek script is a thing of little consequence. But to assume that Wulfila failed to signal a difference between two phonemes when a means to signal this difference lay easily at hand is not a thing of little consequence. Thus the evidence for the double value of *ggw* is only comparative, and I do not believe that anyone, in default of the comparative evidence, would advance the theory that *ggw* had two values. Yet the comparative evidence is to be accorded greater weight than the internal evidence if it is shown that the sound change necessary for the assumption of a single value is improbable (cf. above). Our problem then consists of showing that the change *\*uū* (or *\*ggw*) to /ngw/ is probable or improbable.

Geminated *p, t, k, b, d* (except in *ddj*), *q, hu* do not occur in Gothic except in loan-words (4.11.3). *þþ* occurs in three words and in words where it has been assimilated to an *h*. This shows that clusters such as *dd* and *gg* were uncommon types of clusters from the point of view of the Gothic pattern. It is natural that analogy should enter in in the case of such unusual clusters.

It may be objected to any explanation of the change of *\*ggw* to /ngw/ by analogy that the cluster *ddj* did not change by analogy, and that it was fully as unusual as *\*ggw*. Our task is then to determine why the cluster *\*ggw* should be affected by analogy, whereas the cluster *ddj* was not. The cluster *ggw* occurs a total of 78 times in 12 lexical items. (It occurs 47 times in the supposed value of /ggw/). The cluster *ddj* occurs in 4 lexical items (*daddjan, -waddjus, twaddje, iddja*), a total of 16 times in the

<sup>49</sup> Cf. A. M. Sturtevant, "*Bliggwan, blaggw: bluggwum, bluggwans*", *GR*, VI (1931), 66-67, who believes that *bliggwan* must still belong to the 2nd class. He does not explain the *ggw* of *blaggw*. Braune-Helm, p. 97, considers it to be of the 3rd class.

first three, but over 60 times in M, J, L, Mc. in the last. The fact that this cluster is so frequent in this very common word gives it a functional load much greater than that of the (supposed) cluster *ggw*. It is not too farfetched to think of analogy in the case of this cluster. If one still denies the probability of the change, of course the comparative evidence must be given the greater weight, and we must assume that Wulfila used the graph *ggw* in two values. This weakens our case for *ai* and *au* somewhat, since it shows that Wulfila could use a single sign in two values; i.e. to signal two phonemes.

In view of all these considerations it seems better to give up the theory that *ggw* had two values in Gothic.

5.11.2.c — *h*. Etymology tells us that *h* must have been a velar spirant in some positions in Proto-Germanic, since it has this value in some positions in the older stages of the other Gmc. languages (cf. Gothic *jah*, OHG *jah*; Goth. *nahts*, OHG, OS *naht*, OFr. *nacht*, OE *neah*). Internal evidence had already afforded us this information (4.32.c). There is nothing further to add to our previous discussion here.

5.11.2.d — *s* and *z*. A comparison of Gothic *hūs* with OICEL., OE, OHG, OS *hūs* and Gothic *razda* with OE *reord*, OHG *rarta* shows us that we must reconstruct a voiceless sibilant as the Proto-Germanic reflex of Gothic *s* and a voiced sibilant as the Proto-Germanic reflex of Gothic *z*. Etymology thus agrees with the other evidence (4.32.d and 2.32.d).

5.11.2.e — *l*, *m*, *n*, *r*. Comparative evidence points to the same values arrived at through other criteria (4.32.e and 2.32.e) (Gothic *lagjan*, OIcel. *leggja*, OE *lecgan*, OHG *leggen*; Goth. *guma*, OE *guma*, OHG *gomo*; Goth. *sunus*, OE, OHG, OS *sunu*; Goth. *rauþs*, OIcel. *rauþr*, OE *rēad*, OHG *rōt*).

5.11.2.f — *f*, *þ*. Comparative evidence (Goth. *fadar*, OE *fæder*, OS *fadar*, OHG *fater*; Goth. *þankjan*, OE *þencan*, OS *þenkian*), agrees with other evidence (4.32.f and 2.32.f).

5.11.2.g — *q*. Etymology tells us that *q* was a *k* plus *w* in Proto-Germanic (Goth. *qens*, OIcel. *kvān*, OE *cwēn*, OS *quān*). Internal evidence points to this also (4.32.g).

5.11.2.h — *hu*. Etymology tells us that *hu* was an *h* plus *w* in Proto-Germanic (Goth. *huas*, OE *hwā*, OS *hwē*, OHG *hwer*). Internal evidence has already shown this (4.32.h).

5.11.2.i — *w*. Comparative evidence indicates that the Proto-Germanic reflex of Gothic *w* was a non-syllabic allophone of the \*/u/ phoneme. We have been led by internal reconstruction to postulate this state of affairs for Pre-Gothic (4.21.g). Etymology thus supports our reconstruction, but adds nothing to our previous discussion.

5.11.2.j—*j*. Comparative evidence indicates that the Proto-Germanic reflex of Gothic *j* was a non-syllabic allophone of the \*/i/ phoneme. This has already been postulated above for Pre-Gothic on the basis of internal reconstruction (4.21.f). Etymology offers welcome support for our reconstruction, but adds nothing to the previous discussion.

### 5.11.3 *Length*

The problem of the perception of length is an extraordinarily complicated one, and our discrimination of “length” will usually depend upon what we have been conditioned to perceive as “length”.<sup>50</sup> In those countries where the Latin tradition is prevalent, length is usually associated with closeness and shortness is associated with open quality.<sup>51</sup> This is a concomitant of the fact that the Classical Latin long vowels became close in Vulgar Latin, whereas the short vowels became open vowels.<sup>52</sup> Since the grammarians spoke of the close vowels as “long” and the open vowels as “short”, it was natural to think of the native close vowels as “long” and the native open vowels as “short”. Just as we, following the tradition of another day, speak of the *i* in *bin* as “short” and the *i* in *pine* as “long”, in spite of the fact that one is a monophthong and the other is a diphthong, and that they are articulated in different places. Some scholars, seduced by this tradition, even wish to introduce length as a phonemic feature of American English, though American scholars have shown the fallacy of this by machine measurements.<sup>53</sup>

The usual idea of length in the older Germanic languages, OE, OHG, OS, ON, is based on the accentuation of texts in these languages.<sup>54</sup> It has been demonstrated by Keller and others that the accents in the OE manuscripts are intended to express things other than length, and their arguments have been accepted by several author-

<sup>50</sup> Cf. E. Fischer-Jørgensen, “Objektive und subjektive Lautdauer deutscher Vokale”, *Arch. f. vgl. Phon.*, IV (1940), 1-20; F. M. Henry, “Discrimination of the Duration of a Sound”, *Journal of Experimental Psychology*, VIII (1948), 734-743.

<sup>51</sup> Cf. E. Fischer-Jørgensen; A. Meyer, “Das Problem der Vokalspannung”, *Die neueren Sprachen*, XXI (1913), 169 f.; Haudricourt and Juilland, p. 19. It should be noted, of course, that the lower a vowel is, the longer it will be, cf. B. Malmberg’s review of M. Durand’s “Voyelles longues et voyelles brèves”, *SL*, III (1949), 41. It seems that the perception of length is conditioned by the native language of the perceiver (cf. footnote 58, below). For a full discussion of the problem of length perception and previous studies, see B. Malmberg, “Die Quantität als phonetisch-phonologischer Begriff”, *Lunds Univ. Arsskrift*, New Series I, 41, 2 (1944), 104 ff. and M. Durand, “Voyelles longues et voyelles brèves”, *Collection linguistique publiée par la Soc. Ling. de Paris*, XLIX (1946).

<sup>52</sup> Haudricourt and Juilland, p. 18 f.

<sup>53</sup> Cf. Durand, p. 25 and *passim* and B. Wilson-Tourtebatte, in an unpublished *mémoire*, reported *ibid.*, pp. 26-30. For American scholars who are not in agreement with this analysis, cf. R. M. S. Heffner, “Notes on the Length of Vowels”, *American Speech*, XII (1937), 128-134 and “A Note on Vowel Length in American Speech”, *Language*, XVI (1940), 31-47; H. A. Rositzke, “Vowel Length in American Speech”, *Language*, XV (1939), especially p. 109; C. E. Parmenter and S. N. Treviño, “Length of Sounds of a Middle Westerner”, *American Speech*, X (1935), 129-133. On the question of length in British English, cf. A. C. Gimson, “Implications of the Phonemic/Chronemic Grouping of English Vowels”, *AL*, V (1945-49), 94-101.

<sup>54</sup> Cf. Sievers, # 8; Braune, # 8, *passim*; Noreen, # 33.

ities.<sup>55</sup> The conception of OHG length usually given in the handbooks is based on one dialect, Alemannic (*Benediktinerregel* and Notker).<sup>56</sup> Since the accentuation practices in OHG must have been brought in by Anglo-Saxon monks,<sup>57</sup> it follows that the accents in OHG manuscripts are probably to be interpreted in the same manner as those in the OE manuscripts.<sup>58</sup> Sievers and Schmitt have shown that the accents in OHG manuscripts, as well as the doubling of the vowels, often express things other than length.<sup>59</sup> E. Wadsten has pointed out that the accents in the OIcel. manuscripts are often used over (etymologically) short vowels, and often indicate things other than length.<sup>60</sup> The use of the Anglo-Saxon signs *y*, *þ*, *d* indicates that the writing of the older Norse languages was also brought in or influenced by Anglo-Saxon scribes.<sup>61</sup> If this is true, we must expect the accents in ON manuscripts to signal the same things as those in OE manuscripts. It should further be pointed out, in case the Anglo-Saxon origin of the accents in OHG and ON manuscripts is disputed, that the Latin mss. do not use accents only as a mark of length, but for other purposes — to indicate that a vowel is to be given its full value in unaccented position, as a mark to differentiate words, etc.<sup>62</sup> Even if we assume that Proto-Germanic had phonemic length, we must conclude from the internal evidence (4.21.1) that length was probably no longer phonemic in Gothic. At any rate, the practice of citing Gothic as evidence of length, followed by most of the handbooks, should be given up. Since our ideas of length in Gothic must, of necessity, be dependent on our ideas of length in the other

<sup>55</sup> Cf. W. Keller, "Die Akzente in den ags. Handschriften", *Prager deutsche Studien*, VIII (1908), 153 ff.; Sievers-Brunner, # 8, and the authorities cited there.

<sup>56</sup> Cf. E. Mensel, "Zu den langen Flexions- und Ableitungssilben im Abd.", *JGPh*, IV (1902), 25; Paul Sievers, "Die Akzente in ahd. und as. Handschriften", *Palaestra*, LVII (1909), 1; Braune # 8, # 307, *passim*.

<sup>57</sup> Braune, # 8, Anm. 2; Pribsch and Collinson, p. 402 ff.; Wattenbach, p. 361 f. Since accentuation had long since been given up on the continent, it follows that OHG scribes must have gotten the practice from England or Ireland. Mensel, p. 26, denies the influence of Anglo-Saxon scribes, as does Sievers, p. 11, but to do this, they must suppose that the OHG scribes invented the system of accentuation they use or that they derived their rules from the Latin grammarians. It is interesting to note that accented texts are found mostly in Fulda (or in monasteries under the influence of Fulda, cf. *ibid.*, p. 12 ff.) and St. Gallen, just those monasteries where Anglo-Saxon monks were most active (cf. Pauls Grundriss I, p. 261; J. H. Gallee, *Altsächsische Sprachdenkmäler* [Leiden, 1894], p. xix). Since many parts of the OS territory were converted by monks from Fulda, it is natural that the practices of Fulda should be found in OS manuscripts (cf. *ibid.*, p. xxxv). This, coupled with the fact that Werden and Münster were founded by Anglo-Saxons, and that Anglo-Saxon monks were present in almost all the monasteries and scriptoria in OS territory, explains the number of accented texts in OS (on the history of the monasteries and the Anglo-Saxon influence, see Gallee's *Einleitung*, especially p. xix ff.).

<sup>58</sup> Cf. L. Schmitt, "Die Akzente in den altengl. Handschriften, mit Berücksichtigung der Akzente im Lateinischen und Althochdeutschen", *Bonner Dissertation* (1907), p. 3-9.

<sup>59</sup> P. Sievers, p. 7 f., pp. 12 ff., especially p. 15; Schmitt, *passim*.

<sup>60</sup> E. Wadsten, *Fornnorska homiliebokens ljudlära* (Uppsala, 1890), 122 ff.

<sup>61</sup> Cf. E. Gordon, *Introduction to Old Norse* (Oxford, 1927), p. 246; Noreen, *Altisländisch*, # 22.

<sup>62</sup> Cf. Keller, p. 155 f.; Mensel, p. 26. It is usually assumed that the custom of accenting texts came to England from Ireland (cf. Sievers-Brunner, # 8). It should be remarked, in spite of Keller's statement to the contrary, that Irish scholars are as much at a loss to explain the accent marks as are Germanic scholars (cf. R. Thurneysen, *Altirisches Handbuch* [Heidelberg, 1909], I, p. 18 f.).

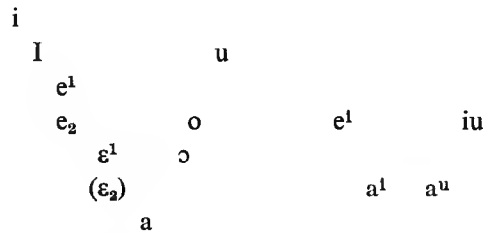
Germanic languages (using the methodology usually adhered to), to use Gothic as proof of the length of a particular vowel in another Germanic language is to argue in a circle.

#### 5.11.4 *Stress*

Comparative evidence indicates that stress was not phonemic in Proto-Germanic.<sup>63</sup> Gothic reflexes of Proto-Germanic prefixes show that this situation must have been changed in Pre-Gothic (4.21. m), since, from a Gothic standpoint, stress was not predictable in Pre-Gothic. Comparative evidence must be given less weight than internal reconstruction, since it is less directly connected with Gothic itself (cf. above, p. 116 f.).

#### 5.11.5 *Conclusions*

The following conclusions are to be drawn from the preceding discussions. Previous investigators, especially those who have (tacitly and explicitly) insisted on giving the greatest weight to etymological considerations, have not been consistent enough in applying their methodology. The best argument against overemphasizing etymological evidence is to apply the (implicit) methodology in all similar cases. From the application of the traditional methodology would result the following system of Gothic vowels, if we disregard length:



To assume such a system, we have to assume that Wulfila did not bother to signal phonemic differences, even when a means lay readily at hand. Very few scholars would be willing to make this assumption to the degree that it would have to be made to justify the system of vowels charted above. Yet this is where the usual methodology, rigorously applied, leads us.

Etymology can tell us some things about Pre-Gothic which we could not know from internal reconstruction. It can tell us that the Proto-Germanic reflexes of *e*, *ei*, *i*, and *u* were two sounds, respectively. In these cases, most scholars have denied that

<sup>63</sup> This is to be seen from the description of the stress patterns of Proto-Germanic in the handbooks (cf. Priebsch-Collinson, p. 82; Hirt I, # 89). That things which are predictable are non-phonemic (or "non-emic") seems to me to be the basic tenet of structuralism; cf. K. L. Pike, *Phonemics*, p. 95 a: "Any item which is completely conditioned or PREDICTABLE is nonphonemic."

these two sounds, respectively, have been retained in Gothic. In all other cases, etymology tells us nothing we have not already learned from internal evidence. We have already said that evidence of alphabet, Biblical names, and system configuration must have greater value than the evidence provided by morphophonemic alternations. It follows that etymological evidence must also have less weight than these, and, where they disagree, than the evidence provided by morphophonemic alternations. It was only in the case of *hu* that the evidence of morphophonemic alternations and etymology was needed to tell us the (approximate) pronunciation of the Gothic sign. It does not follow, however, that we must discard the witness of etymology in all cases. In cases where other evidence is lacking or unclear, we must still use the criterion of etymology. It cannot be too strongly emphasized, however, that the chronology of changes in the other Germanic languages, even when they agree one with the other, should not be allowed to influence our conception of the chronology of changes in Gothic. The fact that parts of the ‘great vowel shift’ and some German and Dutch diphthongizations are parallel does not mean that these developments are connected, or that there was a tendency towards diphthongization in Proto-West-Germanic; the similarity is merely fortuitous, since the causes are different for each language. With such examples before us, we should be careful in the conclusions we may wish to draw from etymology.

We can summarize the results of the previous discussions by the following chart, which represents the phonemes of Wulfila’s Gothic:

*Consonants*

	Labial	Dental	Velar	Glottal
Stops (voiceless or fortis)	p	t	k	
Stops and/or Spirants (voiced or lenis)	b	d	g	
Spirants (voiceless or fortis)	f	þ		h
Nasals	m	n		
Laterals		l		
Vibrants		r		
Semi-vowels	w	j		

*Note:* The signs *q* and *hu* are here considered to be sequences of *k* and *h*, respectively, plus *w*. Another analysis is possible.



*Vowels*

		Front	Central	Back
<i>High</i>	Close	i		u
	Open	I		
<i>Mid</i>	Close	e		o
	Open	ɛ		
<i>Low</i>			a	

It should be remembered that all these sounds are only relative, and that the designations "labial", "velar", "dental", cover a lot of territory. We must be vague in our definition of the phonetic value of the signs, since Wulfila did not, with one exception,<sup>64</sup> indicate subphonemic variants. We cannot even be sure that our phonetic designation corresponds to the modal segment of the phonemes. Wulfila's alphabet indicates the phonemes of his dialect; we can determine the relative position and some of the articulatory features of these phonemes, but this is as far as we can go.

5.2 *AI* AND *AU* IN INFLECTIONAL ENDINGS

In the previous discussions, we have dealt with the pronunciation of Wulfila's signs without regard to their position in the word or in the morpheme, and without regard to what type of morpheme they occurred in; in other words, we have discussed the pronunciation of both stem syllables and of inflectional syllables. In the following discussions, we deal only with the inflectional endings.

If we interpret *ai* and *au* as monophthongs, as we have above, it seems even more probable that *ai* and *au* in inflectional endings may be reflexes of IE and Germanic monophthongs, as well as diphthongs. This reasoning has led Hirt to postulate a different origin for the *-au* of the first person singular optative and the 3rd person plural imperative, as well as the *-au* of the entire optative of the passive.<sup>65</sup> To do this, he has to posit a development of IE *\*-ōm* (Germanic *\*ōn*) to Gothic *au*. Hirt's hypothesis is, as he himself says: "ebenso sicher, wie nur etwas (in comparative linguistics) sein kann."<sup>66</sup> I know of no attempt to derive *ai* in inflectional endings from an IE or Germanic single vowel.

<sup>64</sup> This is his practice of writing *g* for /n/ before *g*, *k*, *q*. Since the velar nasal occurs only before velar sounds, and *n* never occurs there, the velar nasal must be a member of the same phoneme that *n* signals; cf. H. Penzl, "Orthography and Phonemes in Wulfila's Gothic", p. 229.

<sup>65</sup> Hirt I, p. 135ff.

<sup>66</sup> *Ibid.*, p. 136.

### 5.3 THE VALUE OF OUR DISCUSSION OF THE PRONUNCIATION OF WULFILA'S GOTHIC FOR COMPARATIVE STUDY

Since the results arrived at above are at variance with the usual idea of Gothic pronunciation, and since some of our reconstructions must inevitably be based on the Gothic evidence, it is natural that we should wish to re-examine our reconstructions in the light of these results. In discussing the value of Gothic for comparative study of inflectional endings, however, it cannot be our purpose to study the endings in each Germanic and Indo-European language, however desirable this may be, methodologically speaking. To do this would entail writing for each language such a study as was written in our first four and a half chapters. Although much of value might be derived from a re-examination of the evidence for each IE language, this cannot be included in this doctoral dissertation. This part of our chapter must then be restricted for all practical purposes to a critique of the overall views proposed by others. There can be no attempt to replace these views with others here.

#### 5.31 *The "Dreimorengesetz"*

The prevailing theory accounting for developments of inflectional endings from IE to Germanic to the various Germanic languages is the theory of "Dreimorigkeit". I know of no recent handbook which does not (at least tacitly) accept this theory. This theory, as it is set down by its outstanding exponent, Hermann Hirt, is as follows:<sup>67</sup>

Es gab im Idg. zweimorige und dreimorige Längen. Letztere sind meist durch Kontraktion entstanden. Der Unterschied zeigt sich im Griech. als Akut und Zirkumflex, im Litauischen als gestossener und schleifender Ton ..., im Indischen als metrisch ein- und zweisilbig gebrauchter Vokal.

Im Germanischen sind nun beide Längen um je eine More gekürzt, so dass die alten Längen als Kürzen, die dreimorigen Längen als gewöhnliche Längen erscheinen.

Another factor is usually added to the theory of "Dreimorigkeit", however, as in the following discussion by Prokosch:<sup>68</sup>

Roughly speaking, this may be said: during the first two or three centuries A.D., i.e. soon after the Germanic accent shift, final syllables lost one mora. About five hundred years later a second mora was lost; another five hundred years later, a third.

This is translated for us by Pribsch and Collinson into terms of Gothic and the other branches:<sup>69</sup>

<sup>67</sup> Hirt I, p. 133.

<sup>68</sup> Prokosch, p. 133.

<sup>69</sup> Pribsch and Collinson, p. 77.

In many cases the passage from Indo-European to Gothic is characterized by the loss of one 'mora', that from Gothic (as representing Primitive Germanic) to North and West Germanic by the loss of a further 'mora', so that the ratio IE:Goth:W.Gmc. is represented by 1:0:0, 2:1:0, 3:2:1 respectively ...

### 5.31.1 Critique of the "Dreimorengesetz"

The theory as thus set down by its adherents is remarkably clear and concise. It suffers from two fundamental faults, however; a number of exceptions disturb this "neat system,"<sup>70</sup> and its exponents have not defined what "Dreimorigkeit" actually is. In the above quotation, Hirt equated "dreimorige Längen" in IE with the Greek circumflex and the Lithuanian "Schleifton". Since the Greek circumflex indicates a falling intonation and the Lithuanian "Schleifton" is rising, this comparison does not help much.<sup>71</sup> If Hirt chooses to consider the situation in Lithuanian as original, he must explain the reversal of the accent in Greek, and vice versa. This he does not propose to do. It should be further pointed out that, since scholars are in disagreement as to the nature of the Lithuanian accent, especially the "Schleifton", anyone using the term *Schleifton* in a scholarly publication should either indicate what position he is taking or indicate his eclecticism.<sup>72</sup>

If the term "Dreimorigkeit" is taken to mean "super-length", as it commonly is, we may point to the extreme lopsidedness of a phonemic system which has three degrees of length in final position, but only two elsewhere, and to the fact that three degrees of length are unusual in languages, if indeed such a case ever occurs.<sup>73</sup> Some

<sup>70</sup> Priebisch and Collinson, p. 77.

<sup>71</sup> At least the Lithuanian "Schleifton" is described as rising in most Lithuanian grammars (cf. A. Senn, *Litauische Sprachlehre* [Heidelberg, 1929], p. 13; A. Leskien, *Litauisches Lesebuch*, p. 128; but cf. footnote 72, below). On the Greek accent, cf. C. D. Buck, *Greek and Latin*, p. 162f. A. Schmitt has made several attempts to connect the two intonations, by assuming for both a difference of stress, rather than one of pitch (cf. Th. Baader, "Der Intensivierungsverlauf des germ. Akzentes", *Mélanges van Ginneken* [Paris, 1937], p. 232; A. Schmitt, "Schleifton und Stosston", *ZfPhon.*, IV [1950], 90-105). He has failed, since it is certain that Greek had a pitch accent. Cf. also J. Kurylowicz, "L'indépendance historique des intonations baltiques et grecques", *BSL*, XXXV (1934), 24-34. K. H. Meyer, *Slavische und indogerm. Intonation* (Heidelberg, 1920), section 2, says: "... die beiden Akzente: Akut und Zirkumflex bedeuten genau das Gegenteil von dem, was sie im Griechischen ausdrücken."

<sup>72</sup> Gerullis (1930) was the first to demonstrate that the difference between "Schleifton" and "Stosston" in Lithuanian was one of stress, rather than pitch (reported in Th. Frings, "Der rheinische und der litauische Akzent", *PBB*, LVIII [1934], 110-149). Sievers heard the "Schleifton" as rising, falling, then rising (cf. *Grundzüge der Phonetik*, 5th ed. [Leipzig, 1901], section 607). R. Ekblom, *Quantität und Intonation im zentralen Hochlitauischen* (Uppsala, 1925), p. 40f., showed that the "Schleifton" was often shorter than the "Stosston", so that the difference was not one of length, as De Saussure ("Accentuation lituanienne", *Recueil des publications scientifiques*, V [1896], 526-538) had thought.

<sup>73</sup> On the problem of the existence or non-existence of three degrees of length, cf. N. S. Trubetzkoy, "Die phonologischen Grundlagen der sogenannten 'Quantität'", *Scritti in onore di Alfredo Trombetti* (Milan, 1938), p. 156; B. Malmberg, "Bemerkungen zum qualitativen Vokalsystem im modernen Französischen", *AL*, III (1942-43), 46. It is often said that Estonian has three degrees of quantity (cf. B. Collinder, "Three Degrees of Quantity", *Studia Linguistica* [1951], 28-43). P. Ariste maintains, however, that it may be "not so much real quantity as different pitch", and Trubetzkoy seconds him (cf. further P. Eringa, *Het Phonologische Quantiteitsbegrip* [Leiden, 1948], p. 251 ff.).

have explained “Dreimorigkeit” as a rising intonation as opposed to the falling intonation of the usual long vowel.<sup>74</sup> Against such a theory we may raise the objection that it also entails an extreme lopsidedness in the structure, since the rising intonation occurs in (some) final syllables, but nowhere else.<sup>75</sup> It may also be pointed out that the Lithuanian opposition (“Schleifton” vs. “Stosston”), measured by instruments, is not a matter of intonation, but of stress.<sup>76</sup> An explanation of “Dreimorigkeit” as a matter of stress in IE would leave us unable to explain the Greek circumflex. Those who propose “Dreimorigkeit” for IE should define what is meant by the term, since linguists are in disagreement as to what intonation, length, etc. actually are.

The “Dreimorigesetz” was originally proposed to account for certain developments in Germanic. This is especially true for developments in the *u*-stems. If we accept the theory that Gothic *au* signals a vowel cluster, a comparison of Gothic *sunaus* with Lith. *sunāūs* reveals that the Lithuanian circumflex, whatever it signals, is not present in Gothic. If we assume that Lithuanian reflects the state of affairs of IE, Gothic has lost a feature (intonation, a ‘mora’) which Indo-European had. As has been shown above, however, we are not privileged to consider Gothic *au* to signal a diphthong. It is only a historical accident that Goth. /sunəs/ looks like Lith. *sunaus*. Thus we see that Gothic has actually lost two “morae”. The Gothic evidence does not support the theory of “Dreimorigkeit”. The only Germanic evidence which supports the assumption of “Dreimorigkeit” consists of one form in the Benediktinerregel (*fridoo*, with -oo from \*-au, cf. Gothic *sunaus*, J 14,6) and of the accented final -a of the nom. pl. masc. of the *a* and *ja*-stems.<sup>77</sup> E. Mensel has shown that we can put little faith in *fridoo*, especially when we assume that other doublings do not express length, but are merely analogical, as Braune does.<sup>78</sup> Since we have no other evidence in OHG for the length of the final -a of the nom. pl. masc. of the *a* and *ja*-stems, Notker’s accent cannot be taken as absolute proof for its length, especially when his accent may have meant something else.<sup>79</sup> If we disregard the evidence of Notker, we find no support at all in Germanic for the theory of “Dreimorigkeit”. If we accept the length of this final -a, we still will not be willing to change our Indo-European reconstructions for the sake of a handful of forms in one dialect of OHG.

If we find that the extra-Germanic facts warrant the assumption of “Dreimorigkeit”

<sup>74</sup> Cf. above, note 71. Streitberg believed that the final \*i in “schleiftonig” diphthongs could not be lost in Germanic, since the intonation was rising at the end. He made this theory a part of his set-up of “Auslautgesetze” (cf. *Urgermanisch*, p. 228 and *passim*).

<sup>75</sup> This is brought out rather forcibly by E. Hamp in a paper on the Germanic and Scandinavian accent system (to be published shortly in *Acta Linguistica*, cited here with his permission). He wishes to restate the genesis of the “Schleifton” as: “... an intersection of the allophones of a couple of phonemes, each with distribution relatively parallel, or congruent, to other phonemes of its own particular class”. I know of no scholar who maintains that Germanic had intonation on non-final syllables.

<sup>76</sup> Cf. above, footnotes 71 and 72.

<sup>77</sup> Cf. Streitberg, *Urgermanisch*, p. 186

<sup>78</sup> Mensel, p. 33. It should be remarked that any proof based on a *hapax legomenon* is suspect.

<sup>79</sup> *Ibid.*, p. 30f. Cf. above, 5.11.3.

for IE, however, we may be forced to reconsider our position concerning the pronunciation of Gothic *ai* and *au*. It is, for this reason, necessary that we investigate the bases for the theory of “Dreimorigkeit” in the other IE languages.

As we have seen above (p. 96, quotation from Hirt), the theory of “Dreimorigkeit” in IE is based, excluding Germanic, on phenomena found in Lithuanian, Greek, and Vedic Sanskrit. It is assumed that these phenomena derive from some one Indo-European phenomenon — super-length, intonation, juncture, stress. We must investigate each of the languages mentioned to see if this is true.

The phenomenon in Vedic is the fact that some final vowels, for metrical purposes, must be considered as consisting of two syllables.<sup>80</sup> The conditions under which the Vedic hymns have been handed down to us do not, however, permit us to discern in all cases the original state of their meter.<sup>81</sup> Vedic Sanskrit is not known for its fidelity in matters of length.<sup>82</sup> When one adds to this the fact that some final syllables must be read as disyllabic which do not correspond to IE “circumflex” vowels, and that others must be read as monosyllabic which do correspond to IE “circumflex” vowels, it is seen that this criterion is a useless one.<sup>83</sup> One could equally well maintain that IE *\*u* (*\*w*) was a syllabic, since it often counts as one syllable in Vedic.<sup>84</sup> The criterion of metrically disyllabic vowels in Vedic can tell us nothing, since it is ambiguous. At any rate, we are not privileged to consider the metrically disyllabic vowels as being analogous to the Greek and Lith. circumflex vowels, since we have no way of knowing what intonation they had (if they actually existed). Most of the recent handbooks, evidently for reasons like the above, leave Vedic out of consideration and base the theory on Greek and Lithuanian alone.

Since the Greek and Lithuanian developments in final syllables are usually lumped together in the handbooks, we shall treat them together here. The scholar who has been most persistent and effective in his opposition to the theory of “Dreimorigkeit” in IE is Jerzy Kurylowicz. In numerous articles, most of them published in Polish journals, he has sought to demonstrate that the “Schleifton” of Lithuanian and the circumflex of Greek are two different things, and that they do not go back to IE.<sup>85</sup>

<sup>80</sup> Cf. Hirt, *Indogermanische Grammatik*, V (Heidelberg, 1929), sections 127, 128.

<sup>81</sup> Cf. C. R. Lanman, *A Sanskrit Reader* (Cambridge, 1947), p. 354: “The collectors themselves, however, did not by any means write down the texts precisely as they heard them ... The collectors (writers — redactors — diaskeuasts) have often destroyed the rhythm by putting the texts into the strait-jacket of grammar, and especially by writing the words according to the later rules of *sandhi*.” Cf. also W. Whitney, *A Sanskrit Grammar* (Leipzig, 1879), p. 38: “But also a long vowel is sometimes to be resolved into two syllables — oftenest *ā* into *a* — *a*: this resolution is sometimes historical, but ordinarily purely metrical.” Cf. also P. Eringa, p. 160: “Metrische rekking is ... *niet Phonologisch*.”

<sup>82</sup> Cf. Meillet, *Introduction*, p. 139; Lanman’s notes, *passim*.

<sup>83</sup> This may be seen by Hirt’s treatment of the problem (*Idg. Gr.*, V, p. 186f.). He gives 5 cases where the disyllabic vowels correspond to Greek and Lithuanian circumflex vowels, 3 where they do not, and 7 uncertain cases (this by his own count). As he himself puts it, one has to look at this material rather casually to get anything out of it. He mentions the fact that the optatives in *-ēs* and *-ēt* are never to be read as disyllabic, though they supposedly correspond to “geschleift” syllables in IE.

<sup>84</sup> Cf. Whitney, p. 30; Lanman, *Notes*, *passim*.

Opponents of Kurylowicz' line of reasoning attempted to show that the Greek and Lithuanian circumflex accent actually expressed the same thing, *viz.* an interruption in the continuity of the vowel.<sup>86</sup> The only line of attack which can demonstrate the basic difference between the two accents is to deny that they are derived from one and the same thing in IE, since divergent developments of one and the same thing are possible.

This led Kurylowicz to attempt a historical explanation of the development of accent in Greek and Lithuanian. Using the recently developed technique of internal reconstruction and his laws of analogy,<sup>87</sup> he attempts to determine the accent of IE, from which he must depart in his study. He arrives at a description of the IE accent which is in accord with the description usually found in the handbooks. He points out that the circumflex arose in Greek as a result of a series of analogic changes which led to the fixation of the accent on the last two "morae" of the word, plus the preceding syllable.<sup>88</sup> Thus the genesis of the intonations in Greek is marked by an advance of the accent. In Lithuanian, however, it was a recession of the accent which caused the genesis of the intonations. Whenever the accent recedes and strikes a long syllable, that syllable receives broken ("stosstonig") accent (IE *\*māter̥m*, Skt. *mātāram*, Gk. *μητέρα* :: Lith. *móteri*); if it strikes a short syllable, that syllable remains short (*\*duktēr̥i* > *dùkteri*); but the old accented vowel does not receive intonation, i.e. remains the same (IE *\*suesorm̥* :: Lith. *sēseri*). Before this recession of accent, there was no opposition "Stosston" vs. "Schleifton". It is only this recession of accent which causes intonation to become phonemic in Lithuanian. Kurylowicz comes to the following conclusion:<sup>89</sup>

Le fait que la genèse des intonations grèques entraîne la limitation de l'accent nous semble un argument décisif en faveur de notre thèse ... de l'indépendance historique des intonations baltiques et grèques. En effet, si la limitation est étroitement liée à des contractions vocales donnant origine aux intonations, ce ne peuvent pas être les contractions hypothétiques comme *\*ulq<sup>u</sup>o-ei* *\*ulq<sup>u</sup>ōi* ou *\*ulq<sup>u</sup>o-ōm* *\*ulq<sup>u</sup>ōm*, qui n'ont eu aucun effet limitatif sur l'accent védique et balte. L'intonation de -*ō*, est purement morphologique.

I can envisage no response which critics can make to this demonstration except

<sup>86</sup> The most important articles are: "Le problème des intonations balto-slaves", *Rocznik Slawistyczny*, X (1931), 1-80; "The Development of Greek Intonation", *Language*, VIII (1934), 200-210; "L'indépendance historique des intonations baltiques et grèques", *BSL*, XXXV (1934), 24-34. Articles appearing in Polish journals were not available to me; I have read only "Problems of Germanic Quantity and Meter", *Bulletin of the Polish Academy of Sciences and Letters*, 1950, 29-31. The results of these investigations, and a new methodology of treatment are reported in *L'accentuation des langues indo-européennes*, Cracow, p. 122f.

<sup>87</sup> Cf. A. Schmitt, *passim*; Ekblom, p. 40f.; Buck, *Greek and Latin*, p. 161: Cf. further Prokosch's statement, p. 132: "In modern Lithuanian, and probably also in classical Greek, the acute denotes even pitch, the circumflex changing pitch." This line of reasoning cannot be used, however, since the long "gestossen" vowels have a falling (ergo "changing") pitch.

<sup>88</sup> Developed in the article "La nature des procès dits analogiques", *AL*, V (1945-49), 15-38.

<sup>89</sup> *L'accentuation des langues indo-européennes*, p. 122ff.

<sup>90</sup> *Ibid.*, p. 124.

approbation. It has received the approval of at least one qualified reviewer.<sup>90</sup> There can be no reason for not accepting it here. The theory of "Dreimorigkeit", if we accept Kurylowicz' arguments, has no support in the extra-Germanic languages.

### 5.32 Conclusion

Since the theory of "Dreimorigkeit" has no support in Germanic, and no support in the other Indo-European languages, it should be given up. It is my opinion that our present idea of the Germanic developments of inflectional endings should be reviewed in the light of the above discussions. It is unfortunate that previous treatments have accented developments relative to quantity to the detriment of developments in quality.<sup>91</sup> Perhaps an insistence on the exclusion of quantity would be too extreme; it is my personal opinion, however, that the Germanic developments of Indo-European vowels in all positions may best be explained by assuming that length was not phonemic in the older Germanic languages.

<sup>90</sup> Andre Martinet in *Word*, IX (1953), 282-286.

<sup>91</sup> Cf. in this connection the paper of L. Zabrocki, "Uwagi o rozwoju wygłosu w gockim i nordyjskim w zakresie samogłosek długich i dźwięków", *Reports of the Society of Sciences and Letters of Poznań*, 1947, 146-155. Zabrocki attempts to connect the shortening of the vowels under the acute intonation (he accepts the "Dreimorengesetz") and what he calls (p. 147) "rozszerzenie samogłoski" (a "widening" of the vowel) which resulted from (or at least accompanied) this shortening.

## CONCLUSIONS

The most salient results of the foregoing study may be summarized in the following manner:

(1) Previous investigators have been too optimistic in their findings. We are able to ascertain only certain of the articulatory features of the sounds signaled by Wulfila's signs. In a general way we can describe the range of possibilities and the relative positions of the sounds, though even this is not always possible.

(2) The only criteria which afford us direct evidence as to Wulfila's pronunciation are alphabet derivation and Biblical names, and these must be interpreted with caution. Scribal deviations (and syllabification) afford us evidence only as to the language of the scribes and are difficult to interpret. The evidence afforded by an analysis of the pattern of Wulfila's phonemes proved to be inconclusive. The criterion of morpho-phonemic variants, thought by most investigators to afford us direct evidence as to Wulfila's speech, affords only evidence for internal reconstruction; that is, for pre-Gothic. It was shown that comparative evidence can only be given more weight than internal evidence when to accept the evidence afforded by internal evidence would entail the assumption of a sound change which is improbable.

(3) Wulfila's alphabet is phonemic; that is, each sign stands for one, and only one, phoneme. The one exception is found in the case of the velar nasal, where Wulfila was influenced by the Greek scribal tradition to write a sub-phonemic variant of the /n/ phoneme as *g* before *g*, *k*, *q*. Since *g* (/g/) does not occur before *g*, *k*, *q* according to our interpretation, this is not a grave fault in Wulfila's writing system.

(4) It is believed that the case for a single value for *ai* and *au* in Wulfila's Gothic has been stated more strongly here than ever before. Though absolute certainty is impossible to attain in such matters, it has been shown to be highly improbable that they signaled more than one phoneme each.

(5) The results obtained from the analysis of Gothic sounds were applied to a specific problem in the development of final inflectional syllables from IE to Germanic and from Proto-Germanic to the various Germanic languages: the "Dreimoren-gesetz". It was pointed out that this theory or "rule" has no basis in any branch of IE except Germanic. In the light of our investigations of Gothic and other factors, it was shown that it has no basis in Germanic either. This theory should be given up.



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## POSTSCRIPT, 1972

*Habent sua fata libelli.* When I wrote this work in 1955, I had no intention of publishing it, since I was unhappy with certain aspects of it, particularly the first two chapters and the lack of a chapter on internal reconstruction. Although it was not published, copies of it circulated widely and may be found in various libraries, in Paris, Moscow, Louvain along with many libraries in the United States. Thus, in spite of the urging of friends and colleagues, I saw no reason to publish it, though I did begin a new book on the subject, trying to remedy the defects I have just mentioned. As time went on, it became more and more obvious to me that I ought to publish it in its original form rather than attempt a reworking, and for the following reasons: 1. Since the treatment of problems is in terms of method rather than result, the dissertation is not seriously out of date. Although there have been numerous articles written on the subject of Gothic phonology since 1955, most of them have offered positions on methods and combinations of methods, rather than new methods *per se*. 2. With the bibliographical tools now available, bringing the work up to date from a bibliographical standpoint would have been superfluous. 3. This dissertation is well-suited as a milestone in the study of Gothic; it indicates pretty much what our thinking was in 1955 on the subjects it treats. It has been seminal in that other studies have grown up around it and out of it. Seminars have been held on it in such widely divergent places as Paris and Los Angeles. It is a great source of pride to me that Fernand Mossé prepared a French translation of it before his untimely death, and that he was willing, in the second edition of his *Manuel de la langue gotique*, to modify some of his views in conformity with it. Not that I would wish to affirm with Zhirmunsky that my views have become dogma or, with Vennemann, speak of "the spell cast by Marchand", for I merely took to their logical conclusions principles set down by others. 4. Occasionally, because of what Robert Merton, in his *On the Shoulders of Giants* (New York, 1965), has called the "palimpsestic syndrome", views which I was the first to propose have been attributed to others, and I am afraid that use has been made of this dissertation on occasions without explicit mention of this fact having been made. At any rate, pride made me wish to present it in its original form, so that it might take its place in the history of Gothic studies as it was written. I must thank my dissertation adviser, Herbert Penzl, and my second reader, Hans Kurath, for their counsel and aid. Finally, let me thank Mouton and Co. for their patience and the manner in which they have dealt with an extremely difficult manuscript. *Unte qima, gaumei saggwa boko, laisseinai.*

## ADDENDA & CORRIGENDA, 1972

*Introduction.* If I had written this introduction in 1972, I would have had to point to *three* major revolutions in linguistics, for the generative-transformational approach championed by Noam Chomsky qualifies in every way as a revolution, although it does have some antecedents. Application of generative grammar would in no way modify the conclusions of this dissertation, however, since we have between the structuralism of 1955 and the generative grammar of today what Thomas Kuhn has termed a "paradigm conflict", that is, the questions asked are simply not the same. This does not mean, by the way, that the two approaches are necessarily in conflict, just as one can accept both Newtonian mechanics and Einstein's relativity. Generative-transformational grammar does not of necessity invalidate the results of previous investigations. In the present case, it is obvious that the attachment of the phonological component in a generative grammar of Gothic is facilitated by the use of syllable nuclei such as *ii* for *ei* ('long' *i*), as in Buckalew's approach (Ronald E. Buckalew, *A Generative Grammar of Gothic Morphology*, U. of Illinois Dissertation, 1964), or that the statement of certain grammatical rules may be made easier by the assumption of an extra feature (called length) for certain vowels (Theo Vennemann, *Language*, 47, 90-133), but this cannot change the so-called taxonomic phonemics of Gothic, the object of this dissertation, which would be the surface input of a transformational grammar of Gothic. As far as the pronunciation of Gothic is concerned, transformational grammar can do little to help us, although it does permit more elegant statements of the morpho-phonemics and is, of course, of great value to internal reconstruction.

1.02. I have since come to the conclusion that there is no evidence that Wulfila was familiar with the runes, cf. my "Les Gots ont-ils vraiment connu l'écriture runique?" *Mélanges Fernand Mossé*, Paris, 1959. I should point out that an attempt has been made recently to derive the Gothic alphabet from the runes (L. A. Viehmeyer, *Gothic Scribal Tradition*, U. of Illinois Dissertation, 1971).

2.10. On the question of the *Vorlage* of the Gothic Bible, it should be pointed out that some investigators believe the genealogical *method* used by previous scholars to determine textual affinities in the Greek manuscripts is invalid, and that attacks on its results continue, particularly under the influence of E. C. Colwell (cf. his contribution in *Early Christian Origins*, ed. A. P. Wikgren, 1961, conveniently summarized

by Robert M. Grant in *Religion*, ed. Paul Ramsey, N.Y., 1965, 122ff.). In his *Gothic Studies* (Medium Aevum Monographs, No. 6, 1961), G. W. S. Friedrichsen has tried to determine Streitberg's method, which was implicit rather than explicit. It was fatal for Streitberg's reconstruction of the *Vorlage* that he accepted von Soden's text types.

2.20. We are no closer today than we were in 1955 to a knowledge of the pronunciation of 4th century Greek. If I ever rewrite this chapter, I intend to take the grammatical traditions of the 4th century, the "century of grammarians", into greater account. I hope that I have demonstrated ("Das akrophonische Prinzip und Wulfilas Alphabet", *ZfdA* 86 (1956) 265-275) that Wulfila followed the acrophonic principle, the usual grammatical tradition of his day. That is, he used each Greek sign in the value of the first sound of its letter name, as was done throughout the nineteenth century in Europe and America, where it was even proposed that the name *aitch* for *h* should be changed to *haitch*, so that students might learn it more easily. We can thus explain the use of *epsilon* to render *eta* in Biblical names, since the sound of its letter name was close *e*. Wulfila must have been familiar with a letter name [wi] for *upsilon*, the source, e.g., of our modern English letter name for *y*, and this accounts for his use of *upsilon* in the value of [w]. If we derive Wulfila's *u* from Greek *omicron*, we can thus explain the use of *omicron* to render [u], since its letter name was [u].

3.0. It is unfortunate that we do not have studies of orthographical deviants in a modern language, where their significance could be determined. A study of spelling errors by college freshmen, for example, reveals a high percentage of random errors in writing vowels, such as *i* for *e*, *u* for *o*, where there is obviously no correlation with their pronunciation. Today, I would be even less optimistic than I was in 1955 about the value of scribal deviants in determining pronunciation. This is particularly true of syllabification (3.2); I do not know why I accepted the position of previous scholarship to the effect that breaking of the type *i-u* demonstrated that *iu* was a diphthong. If it means anything, it would have to mean that *i* and *u* were in hiatus.

3.02. On the matter of dialect mixture in our Gothic manuscripts, see my later article: "Dialect Characteristics in our Gothic Manuscripts", *Orbis* 5 (1956), 141-151, where a method is proposed to distinguish Wulfilian variations from scribal ones.

3.05. On Uppström's "nefarius corruptor" see now J. Victor Johansson, "De Rudbeckianska Förfälskingarna i Codex Argenteus", *Nordisk Tidsskrift för Bokoch Biblioteksväsen*, Arg. 42 (1955), 12-27, who suggests the well-known Rudbeckian Carl Lundius as the culprit.

4.10. If one were to translate Trubetzkoy's *phonologisches Sprachbewusstsein* by "deep structure", his statement would make sense in modern terms. For those interested in the history of linguistics or the translatability of structural statements into generative-transformational terms, it might be interesting to note that, throughout, my *feature* is roughly equivalent to *distinctive feature*, although its derivation in the deep structure of theory is different. Likewise, my *loss*, *introduction*, *assimilation*, etc. could be translated as *rule simplification*, *rule reordering*, etc., but *cui bono*?

4.20. In a later article, I pointed out that French *saurai-savoir* offered a perfect



refutation for those who wished to take the alternation *au-aw* (as in *gawi-gaujis*) in Gothic as proof that *au* was pronounced as a diphthong. A. van der Lee ("Zur Aussprache der gotischen Digraphen *ai* und *au*", *Festgabe für L. L. Hammerich*, Copenhagen, 1962, p. 131, footnote 1) misunderstood my point and assumed I was referring to the French *spelling*. It was rather to pronunciation that I referred, since this demonstrates that an alternation *vowel-vowel/semivowel* does not demonstrate that the vowel is a diphthong, since we know in this case that it is not.

4.21.1. In a recent article, Theo Vennemann (*Language* 47 (1971) 90-133) has tried to use these patterns to demonstrate that length was phonemic and phonetic in Gothic. It must be emphasized, however, that the choice of *ei* or *ij* is not phonetically or phonemically conditioned, cf. our list of exceptions. That Vennemann does not understand "taxonomic" phonemics is evident from his continual use of prediction in terms of grammatical categories, such as verb and noun, and other categories, such as reduplicative affix, explicitly excluded from most classical phonemic theories. I have no objection to this change in the rules of the game; it should be explicitly stated that this is a change in the rules.

#### Errata

p. 19, l. 19. The Vatican codex cited here may not exist. In preparation for his dissertation (cf. above, 1.02) L. A. Viehmeyer wrote to the Vatican to ask for a photograph, but no Gothic alphabet was found in the manuscript where Pertz indicated it to be.

p. 22, footnote 40. The Veronese marginal notes offer an example of scholarship at its worst. It was not until 1947 that scholars were even able to affirm that they were written in the Gothic alphabet! I contributed, *mea culpa*, to this shoddy treatment by not noting E. A. Lowe's publication of a photograph of one of the notes, a fault I remedied in *JEGPh* 56 (1957), 213-224. I have now made ultra-violet photographs of the manuscript and will publish a tentative edition in *New Testament Studies*, to appear soon. At present, I can only point out that much found in previous editions is simply conjecture, some of which could not have existed. The new leaf of the Codex Argenteus, discovered in Speyer in October, 1970, although it offers many interesting discoveries, has no few forms of the alphabet.

p. 68, l. 30. *hu* does occur before *t*, and I was in error in following previous statements to the contrary, cf. J 8,57 *sahut* and J 9,37 *gasahut*.

p. 98, footnote 75. Hamp's article has since been published: "Final Syllables in Germanic and the Scandinavian Accent System", *SL* 13 (1959), 29-48.